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THE
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THE TARIFF ACT OF 1930

SUMMARY

The final settlement; commanding position of the Conference Committee. — Higher duties on agricultural commodities, such as sugar, wheat, cotton, 3. — Hides, wool, meat, and dairy products, 7. — Minor agricultural products, 9. — Manufactured articles, cottons, woolens, silks, 13 — The general trend, 16. — The Tariff Commission retained, with no marked changes in its powers, 19.

In this Journal for February, 1930, I gave an account of the tariff situation as it then stood. The bill had been passed by the House, and was under consideration in the Senate as reported by the Finance Committee of that body. The "insurgent" Senators had begun a move toward reduction of rates, but had not stuck to it. The situation was chaotic; and so it remained to the end. The bill as finally passed by the Senate reduced some duties below the House rates, increased others. Both bodies left a considerable number untouched. As usual, the measure went to a Conference Committee, and the innumerable cases in which the two houses differed were settled there. The Committee sessions were necessarily compressed into a few days. Everyone was weary and in a hurry to get through. Amendments were compromised, with a leaning here to the House rates, there to those of the Senate. The nature of the outcome — whether in the end a rate higher or lower

than the corresponding one of 1922 — depended on compromise, "trading," accident, and not infrequently on the persistence or dominance of some individual member.

The practise of final settlement by a Conference Committee has become, like the selection of the chairmen of congressional committees, part of the unwritten constitution. It does not always work badly. When the main lines of a legislative scheme have been worked out, but differences have arisen on details which, tho important, are not vital, the process of reaching a final settlement by adjustment and compromise may well be left to a committee made up of the leaders and the experienced. Such was the case, for example, with the Federal Reserve Act, nearly twenty years ago; such again was the case with the Boulder Dam Act of 1929. But on the tariff a multitude of details are involved, and in most cases these are the vital things, not only for the members of Congress at large, but for the members of the Conference Committee. As regards tariff adjustments and compromises, there is hardly a word of good to be said for the practise. Legislation simply proceeds in haphazard and irresponsible fashion.

This too familiar characteristic of tariff-making, as well as the history of the present measure, needs to be borne in mind when examining the details as finally settled. It was not an unusual history; the same sort of thing has happened in almost every tariff act of the last half century. But the evils were greater this time than ever before. The very length of the session was unexampled. It began in June, 1929; indeed the first informal preparations had begun even earlier. The act did not finally become law until June 17, 1930. No gain in consistency of policy or in discrimination as to details accrued from this prolonged deliberation; on the con-

trary, it was the sign and the result of something like a breakdown in the legislative machine.

It would be wearisome and unprofitable to consider all the details of the final rates, or even those that might call for comment. Indeed, the significance of some among the changes is obscure. One is often led to suspect that the pervading process of log-rolling and swapping has ended in changes which some particular domestic interest and its Congressional representative had at heart. I shall confine the following account of the act to cases that are of economic importance or are characteristic of the policy of the act, — or the lack of any policy.

First, the agricultural commodities. Among these sugar is the most significant. The sugar tax, as need hardly be said, has come to be the most weighty part of the protective system, surpassing in economic and political consequence the duties formerly dominant — those on iron and steel, textiles, wool. It illustrates also, better than any other, the tortuous course of the legislative proceedings, and the absence of any controlling principle.

The duty on Cuban raw sugar — the rate which alone signifies — had been fixed at 1.76 cents per pound in 1922. It was put at 2.40 in the bill as passed by the House. There was some endeavor, when the bill first reached the Senate, to work out a sliding scale, by which the rates were to rise as the price went down, fall as it went up. The plan was known to be favored at the White House, but was dropped after a discussion which was hardly more than perfunctory. The report of the Senate Finance Committee put the duty at 2.2 cents; the Senate Committee of the Whole lowered it to 1.76 cents (the figure of 1922 — this was during the brief interval when the insurgents were in command and

were "slashing" rates); the Senate, when at last it passed the bill, made the figure 2.0 cents; and in the law as finally enacted it became 2.0 cents.

The economic effects of the sugar duty have been much discussed and are sufficiently known.[1] During the decade 1920–30, as in the pre-war years, about one half of the supply has come from Cuba, the other half from the domestic and quasi-domestic (duty free) sources, — Louisiana, the beet-sugar states of the West, Hawaii, Porto Rico, the Philippines. The economic situation is by no means simple, but the main things are clear enough: an industry of great quantitative importance; in a state of depression which in the main is a long continued aftermath of the war; clamoring for protection on which it is partly dependent and partly not; posing as a representative of the farmer, when in fact the great bulk of the farmers are affected only as consumers.

One thing in the sugar outcome is clear. The supposed underlying principle of protection — the equalization of competitive conditions through duties based on differences of cost — was ignored. The sugar producers, especially the beet-sugar people, asked all they thought it possible to get; the Republicans in House and Senate gave them, more or less grudgingly, what seemed necessary to fulfill campaign pledges and to hold their party associates in line. The figures were the result of give and take, manœuvering and compro-

1. I refer again to Mr. P. G. Wright's analysis in his book on Sugar in Relation to the Tariff (1927). The situation remains (1930) essentially the same as he then described it. Indeed, the general economic situation remains as it was described by myself in Some Aspects of the Tariff Question (1915).

I take this occasion for correcting a slip in my previous article. Mr. Wright was there quoted (p. 187 of the issue for February, 1930) as concluding that a duty between 1.50 and 1.75 cents would serve to equalize differences in cost between Cuban and domestic sugar. The figure he did suggest was something between 1.25 and 1.50.

mise. There was no pretense that any one of them was based on the difference in cost between the domestic and foreign producers. The plea was that the price of sugar was low, and that something must be done for the farmers (chiefly in the mountain states) who grew sugar beets. There was no suggestion that the *difference* between the two in cost and price had changed—that there was anything in the relative figures to give occasion for a higher duty; no pretense of applying the principle on which the protective system is now alleged to rest.

Among the other agricultural articles some were important and some trivial; but there was a similarity of spirit in the handling of all.

Among the important articles is wheat. The duty on wheat had been 30 cents a bushel in 1922. In those days it was thought a high rate, and was explicable not only by the then depressed state of farming, but also by the post-war burst of nationalism and protection. The Tariff Commission subsequently made an investigation of costs — the difference of costs between the United States and Canada for hard wheat — and recommended a rate of 42 cents a bushel, which was put into effect (under the flexible provision) in 1924. That figure is retained in the act of 1930 — 42 cents a bushel.

The economic significance of the wheat duty, and the meaning of the Tariff Commission figures of 1923, make a story too long for the present summary account. In the main the case is one of border trade. The duty affects not all the wheat producers, but those in the Northwest who grow hard spring wheat. The only imports are from the Canadian provinces where wheat of the same grade is grown. The crop yields from year to year in the two competing regions vary greatly, and cause the effects of the duty to be very different in different seasons. Only the desire to propitiate the

farmers explains the duty, and I can see no economic justification for it.[2]

Flaxseed is in some ways similar to wheat; similar at least in that the demand for a duty on it comes from the farmer of the Northwest, where the pioneer stage in agriculture has not yet been passed. The duty had already been made 40 cents a bushel in 1922. The Tariff Commission in 1929 recommended (under the flexible provision) a duty of 56 cents, and this the President put into effect. The make-up of the Commission at that time, not to mention the general political situation, made it certain that the difference in cost would not be minimized, and that as high a duty would be recommended as the principle of cost-difference could be strained to justify. But in the act the figure was made still higher — 65 cents. There is a substantial importation of seed, chiefly from Argentina, and competition between the oil-pressers (the buyers of the seed) near the seaboard and those in the interior. The crop presents curious problems, and has a unique place in the agriculture of pioneer regions; it would be going too far afield to enter on them here.[3]

Another important commodity is cotton; and here not only is the commodity important, but the full weight of the duty is certain to be felt. Raw cotton has been free of duty from time immemorial. As regards the kind of cotton grown in the South, it was obvious that no duty could signify. But now cotton

2. Compact and good statements of the main facts in the wheat situation are in J. D. Black, Agricultural Reform in the United States, (p. 210) and an article by P. G. Wright, already mentioned, in The Tariff Review for November, 1929.

3. See an article by W. S. Barker in this Journal, May, 1917, for the persisting peculiarities of the crop; and a compact discussion by P. G. Wright on the immediate situation, in the article in The Tariff Review already mentioned — an article which surveys judicially the current situation for a large number of the agricultural articles.

of long staple — quite a different variety — is subjected to a duty of seven cents a pound. It is used chiefly for cotton knit goods, and has long been supplied solely by importation, mostly from Egypt. The Imperial Valley — that remarkable low-lying region in the Pacific Southwest, transformed from a desert to a garden by irrigation from the Colorado river — has been found advantageous for raising it. The cotton growers there, like fruit growers in California, who have long been insistent on duties for their products, were clamorous to have a share of the legislative favors. The planters of the South at large were naturally lukewarm, but quite as naturally were unready to object.

The item had a curious and instructive career in the legislative history of the act. Cotton had been left free in the bill as passed by the House, still free in the bill as presented to the Senate by the Finance Committee of that body. But in the Committee of the Whole, at the stages when the insurgents were in command, and were determined as well as incensed, the Senate inserted the duty of 7 cents. To the Westerners it seemed that the eastern cotton manufacturers were opposed to a duty which the growers wanted; that seemed to them another in the long list of grievances. The amendment by which a duty was imposed was not eliminated from the bill as passed by the Senate, nor as it ran the gauntlet of the Conference Committee. It is not easy to guess how it came to be retained in the sessions of that potent and secretive body — but incorporated in the act it is.[4]

Another much discussed set of agricultural articles is meat; with it, tho less important, go the dairy pro-

4. I suspect from what has come to my ears that the result was reached in a quiet consultation between the chairmen of the Senate and House Committees, the Conference Committee as a whole accepting what these two brought in.

ducts. Here, too, the farmers get what they want. The duty on cattle goes up from figures running from ½ to 2 cents a pound, to higher ones of 2½ to 3 cents; that on beef from 3 to 6 cents. On sheep the advance is from a duty of 2½ to one of 5 cents, on mutton and lamb from 4 to 7 cents. On swine it is from ½ to 2 cents, on ham and bacon from 3 to 3¼ cents; with which may be noted that on corn, from 15 to 25 cents a bushel. On milk and cream the duties are nearly tripled; on butter and eggs they are raised less. Live poultry also feels a heavy hand — 8 cents a pound instead of 3 cents.

Related to the duty on meat, yet presenting economic problems quite different, is that on hides. Here there are larger and continuing imports, as well as great domestic production. And here, as in previous years, there was divergence of interest between the manufacturers and the producers of the raw material. The shoe manufacturers in general wanted hides and leather to remain free, as they had been in 1922, and were willing that shoes should remain free if hides also remained free. But some manufacturers of particular grades wanted duties on shoes. The situation in the manufacturing industry is of the sort familiar in our industrial development. There is an enormous domestic production; imports are small, yet special qualities continue to come in; for the time being there has been a spurt of larger imports of this sort. The final outcome was a duty of 10 per cent on hides, 15 per cent on leather, 20 per cent on shoes.[5]

5. The vagaries of the session are again illustrated by the ups and downs of the hide duty. Briefly the history was:

In the bill	Duty on hides
as passed by the House	15 per cent
as presented to the Senate by the Finance Committee	17½ "
as fixed by the Senate in Committee of the Whole	free
as passed by the Senate	free
as finally enacted	10 "

To be mentioned with the new duty on hides is an increase in that on wool. The change is not great — from 31 cents to 34 cents per pound of clean wool (this method of levy, introduced in 1922 by which the clean content of wool is the basis of the duty, is retained). That even so inconsequential a change, advancing a duty already high, was thought worth making, is again significant of the general endeavor to placate the Westerners. More important is a higher duty on wool rags; the increase being from 5 to 18 cents. The rags are imported in considerable quantity and are torn apart for making "shoddy" or "reworked wool"; a process to which increased resort has naturally been made as the duty on wool has pushed up its price higher and higher. The advance on rags is a logical and consistent extension of the policy of high protection on the wool itself. Characteristically enough, it was opposed by the representatives of the manufacturing states. The manufacturers have long settled down to an acceptance of high duties on wool as the price they must give for holding those on woolens; but it has always been a grudging acceptance, and this further instalment was a bitter pill to swallow.[6]

Certain agricultural articles listed in the chemical schedule, such as casein, various vegetable oils, potato starch, also were subjected to higher duties. Another

6. The history of this item is a further illustration of the curious twists in the successive stages of the measure.

	Duty on wool rags
In the act of 1922	7½ cents a pound
In the bill of 1930,	
as passed by the House	8 " "
as reported to the Senate by the Committee on Finance	24 " "
as amended in the Senate Committee of the Whole	18 " "
as passed by the Senate	18 " "
as enacted	18 " "

change in the chemical schedule — of a different kind but also significant of the spirit of concession to the farmers — is the relegation to the free list of ammonium sulphate, an important ingredient in fertilizers. The making of synthetic ammonia is a new and rapidly growing industry, such as would ordinarily be fostered under a highly protectionist policy; but here was another chance to curry favor with the farmers. Of the same sort is the continued free admission of agricultural implements — a mere gesture, but of no substantial importance to anyone.

Some of the other changes in agricultural duties are futile, mere camouflage; some mean something but not much; still others have a real significance, even tho they are at present not of great economic effect. Futile are the higher duties on swine and their meat, and on corn. These things are articles of export, not of import. To give the farmers higher duties on them is a continuation of the old process of trying to throw dust in their eyes.[7] Dairy products are in the main a matter of domestic trade only. The duties on them are of significance chiefly for the border trade with Canada — which

7. The Tariff Commission in 1928 went through the motions of making an investigation on the differences in cost of production for corn in Argentina and the United States. A few shiploads of corn — enough for a little chicken-feed — had happened to be sent to New York and San Francisco. Transportation as a cost played a great part in the Commission report. Half of the commissioners, strongly protectionist in spirit, reckoned as part of the "cost" of American corn the transportation charges by rail from the corn belt to San Francisco, which was supposed by them to be the "principal competing market"; and by so loading the American cost, reached a figure of 35 cents as representing the the excess of American over Argentine cost. The other half, less protectionist, treated New York as the principal market, and made the difference 12 cents a bushel. All the figuring was of a dubious sort, and the President, naturally and wisely, did nothing. The duty had been 15 cents a bushel in 1922; it is made 25 cents in 1930. The report of the Commission in this case is worth examining, as an example of ill-directed effort.

is closely akin to the domestie trade between our states. The duties have local effect here and there, and so are not entirely negligible. Yet in their bearing on the nation as a whole and on the farmers as a body they are again hardly more than a gesture. Their important and lamentable effect is to irritate and even incense our Canadian neighbors and seriously jeopardize our friendly relations with them.

Of greater significance are the meat duties (other than on swine). They are of large interest, however, in relation not so much to the immediate tariff situation as in their bearing on larger ultimate problems. A new stage in economic development is setting in: the growing-up of the country to its agricultural capacities. We are approaching a situation where there will be consumption within the country of all that is produced of these things, and cessation of the exports. It is becoming possible, even tho not likely to be on a considerable scale for the next few years, that some part of the meat consumption of the seaboard regions of the East will be supplied from Argentina. Only the very first steps in this transition are discernible by 1930, and years are likely to elapse before it will be carried far. But the question begins to loom up whether the United States is to adopt a policy not merely of fostering manufactures and a varied industrial structure, but one of complete self-sufficiency; a question of wider concern, economic, social, and international, than anything directly suggested in the debates of 1930. The chief thing then considered was the importation of "feeder cattle" from western Canada, whose competition with similar cattle from the American range is another case of border trade, but is thought a dire menace.

There is a long list of changes that were petty, such

as must have made it difficult for the spokesmen of the farmers themselves to keep a sober face. Cabbages, celery, eggplant, lettuce, turnips, green peas and beans, tomatoes, clover seed, mustard seed, all are encouraged by higher duties. So are hay and potatoes; obviously affected by the tariff only at spots along the border, according to the transportation expenses of these bulky things. The duty on tulip and narcissus bulbs is tripled; it raises a smile to see these dealt with as if the farmers were concerned. The Californians are appeased by increases on lemons, limes, grapefruit, plums, and prunes. Onions and tomatoes too are hit hard; here, while some truck-farmers may be affected by early supplies across the southern border, the imports are a negligible fraction in the total consumption.

As I have already remarked, the representatives of the farmers were ready enough to accept and even demand higher duties on each and every petty agricultural product. But after all, they felt that, if there was to be legislation that would really help, it must be in other ways. The accepted line of action being that of direct relief for people in difficulties or supposed to be so, the inevitable demand was that, so far as the tariff could not be of any real service, something else should be done. Hence the insistence for aid to the farmers on similar lines, but effective aid. There must be some sort of bounty if the farmers were to get the essential benefit of the tariff — higher prices for their products than could be got under free trade. They *had* to sell abroad at the free-trade price; then let the equivalent of a tariff rate be secured to them at home. The devices proposed for securing this end were various, and their details need not concern us. It was the debenture plan that was most fought for, first in the Agricultural Marketing Act and then in the Tariff Act. From both it was finally

eliminated. What is significant is the contention of the farmers that they must have, not mere sops, but a real share in favors that were being distributed.

On manufactured articles the proceedings of the session ran on in much the way familiar in tariff history, and the outcome was on the whole of the same sort. While the demands of depressed industries received the promised attention, there was no serious attempt to confine the changes to such cases. This or that manufacturing concern or industry tried to engineer a higher rate, usually with some degree of success. And there was wavering, uncertainty, intrigue, until the final settlement was hurriedly reached in the Conference Committee.

Among the textile industries, the cotton manufacture more particularly presented claims on the ground of depression. Yet so much had been done for it in previous acts, and the structure of high protection had been so systematically built up, that little could be added. The complicated arrangement of duties set up in 1922 is retained, the figures being set somewhat higher. Indicative of the general trend are the ad valorem rates on the finer goods; they had been in general 45 per cent in 1922, and are now run up as high as 62½ per cent, and on some goods (e.g. the finer handkerchiefs) even as high as 67½ per cent. Quite properly, a compensating duty was imposed on goods containing long staple cotton, of 10 cents per pound of such cotton contained in them. The real cause of trouble for the manufacture of most grades of cotton — indeed of all grades that really signify in the domestic industry — is the extraordinary growth in the South; a semi-artificial and almost insensate growth, much promoted by a use of night-work so widespread as to shame our civilization. For the finer grades, the competition of silk and rayon

goods and the fashion of scant clothes for women were the main factors in the depression; and no advance of duties can be a remedy.

For woolens there was less claim on the ground of special depression, at least not as regards woolens proper; tho in this industry also the changes in fashion have brought troubles in all the manufacturing countries. As with cottons, so with woolens, the existing schematic arrangement of the duties is left untouched, but the rates are advanced on the finer goods. On these the ad valorem rate (i.e. protective part of the duty), which had been 50 per cent in 1922, is now made 60 per cent. It is these finer goods alone — the expensive qualities of cloths and dress goods — that are imported in any considerable degree, and the imports are negligible compared to the total domestic production. The trend is the same as has appeared in the long series of tariff acts since 1867: persistent screwing up of the duties on the goods which cannot be made to advantage in this country, and yet failure to achieve the end apparently in view — that of shutting out the very last scrap of imported goods.

On silks the same sort of thing is done, but in this case without its being possible to allege any depression; the industry has prospered and grown mightily through the decade. Most silk goods are left as they have been, but here again the rates are made higher on the few fabrics that continue to come in. Among them the one bulking largest among the imports (and at this only a few millions' worth) is velvets; the rate is advanced from 60 to 65 per cent. The same increase appears on silk apparel, and in the dragnet clause on silk manufactures not otherwise provided for. There is a curious advance from 55 to 65 per cent on certain figured silk ribbons — a small item, imports almost negligible; one wonders

what domestic producer got his congressman to put in this little fillip.

There are sporadic changes on various manufactures. On chinaware they are analogous to those in the textile list. Some grades here also continue to be imported, and in larger proportions than for the textiles. The duty on decorated china, already raised to 70 per cent in 1922, is again increased, by adding to this ad valorem rate a specific duty of 10 cents a dozen. Surgical instruments, put at 45 per cent in 1922, now are 55 per cent (the House bill had even proposed 70 per cent). Scientific glass instruments, on which the duty had been made 65 per cent in 1922, are now set at 85 per cent. On pocketknives there is a petty advance, which is worth noting because indicative of the general petty procedure. Very high and complicated duties had been imposed in 1922.[8] In 1930 they are left untouched except for one item. On the very cheapest knives, worth 40 cents a dozen or less, the specific duty (additional to an ad valorem duty of 50 per cent) had been made 1 cent apiece in 1922. It now is just a bit higher — 1¼ cents. On this lowest grade the compound duty of 1922 (ad valorem and specific combined) had been equivalent to 85 per cent; while on the others the range had been from 95 to 170 per cent. The very high rates on all the better grades had proved prohibitive of importation; but small imports of the cheapest grade still came in (a beggarly $100,000 a year). So here an attempt was made to get a higher duty, in the end with only the slight advance just noted.[9]

8. See my Tariff History of the United States, p. 470.

9. The history of the act shows that some individual producer and his (or their?) legislative representative pressed hard for getting a great increase on this small item. In the bill as passed by the House the specific duty had been 2 cents apiece; as reported by the Senate Finance Committee, 4 cents; as amended in the Committee of the Whole (in-

More important in industrial effect is a large increase in the duties on watch and clock movements — a complicated schedule, administratively bad, with compound duties (partly specific, partly ad valorem) graded according to the value of the articles. A curious case of duties lowered, and one illustrating again the tortuous history of the measure, is that of aluminum utensils; something in which householders were supposed to be interested, and in which the Secretary of the Treasury, dominant in the aluminum monopoly, was supposed also to be interested. The rate in 1922 had been 11 cents per pound plus 55 per cent. The House bill reduced it a trifle, to 11 cents plus 50 per cent; the Finance Committee left it there; the insurgents in the Senate cut it drastically, to 25 per cent flat, and so the Senate left it; finally it emerged from the Conference Committee, and was fixed in the act, at 8½ cents plus 40 per cent — a lower rate than that of 1922, but still amply high enough to keep the imports down to a negligible figure. The metal itself (aluminum) on the other hand, came out with an increased duty. The figure had been 2 cents a pound in 1922. The House bill made it 5 cents, the Senate Committee left it at 5 cents, the insurgents cut it to 2 cents on the floor of the Senate; the Conference Committee settled it at 4 cents.

Some indication of the general trend in the act is given by a comparison of the average ad valorem rates, computed for each schedule. The averages stated below are reached by taking the imports for 1928, computing the duties then actually collected under the act of 1922 and their per cent on the imports, and then computing what would have been the per cent if the duties of 1930 had

surgents!), 1 cent — i.e. the duty of 1922 unchanged; this refusal to advance the rate remained in the bill as passed by the Senate; finally it emerged from the Conference Committee with 1½ cents.

been in force on the same imports. Figures of this sort must be used with care; but they do indicate the direction in which the rates moved. They are taken from a compilation made by the Tariff Commission immediately after the passage of the act. It will be seen that there is some advance in each and every schedule, and that the greatest change is in the agricultural schedule.

AVERAGE RATES, BY SCHEDULES, IN THE TARIFF ACTS OF 1922 AND OF 1930

	Act of 1922 per cent	Act of 1930 per cent
1. Chemicals, oils, and paints	29.22	31.40
2. Earths, earthenwares, and glassware	45.62	53.62
3. Metals and manufactures of	33.71	35.01
4. Wood and manufactures of	7.97	10.49
5. Sugar, molasses, and manufactures of	67.85	77.21
6. Tobacco and manufactures of	63.09	64.78
7. Agricultural products and provisions	19.86	33.62
8. Spirits, wines, and other beverages	36.48	47.44
9. Manufactures of cotton	40.27	46.42
10. Flax, hemp, jute, and manufactures of	18.16	19.14
11. Wool and manufactures of	49.54	59.83
12. Manufactures of silk	56.56	59.13
13. Manufactures of rayon	52.68	53.62
14. Paper and books	24.72	26.06
15. Sundries	21.97	27.39

Looking at the act as a whole, one is struck with a certain futility in it all. What has been said on the several articles indicates how liberally the advances on agricultural products were granted, and why they failed to placate the supposed beneficiaries. In the main they are no more than gestures, made because the Republicans had vaunted the tariff as a never-failing remedy and were under bonds to keep their promises for this sort of relief. As regards manufactures, the long series of preceding tariff acts has left no large body of articles without heavy protection, indeed virtual prohibition. Only cracks and crannies could be found here and there

in the protectionist wall, and the drift is toward closing these, even tho they may be of the smallest. But no further changes of much moment can be made on the manufactured articles.

When I speak of futility in this legislation, the reader must bear in mind that it is said, after the way of economists, with an eye mainly to the permanent or long-run results. When it comes to the turn which things take for the time being, there must be qualifications of the sort which the economist always has in mind, or should have. Temporary and unexpected shifts may occur which will seem to belie his words, and in this case to belie the characterization of futility.

At the very moment of writing, for example, comes the unexampled drought, which has cut down the corn crop and shot up the price. A flurry of this kind is of cardinal importance for the season, even tho over any considerable period it will be seen to be no more than a flurry. Corn may be imported during the current season in substantial quantities; while its export, in the form of swine products, may cease for a year or so. The new import duty may keep up the price for the time being and really help the farmers. The hot partisan who preaches protection as the farmer's remedy will point with pride to this immediate and visible outcome. Very few will stop to consider that the very meaning of a vast market and a world price is that they mitigate the irregularities of crops and supplies, and that this mitigation is a familiar and unquestioned gain. Such it is admitted to be when a failure of the potato crop in one part of the United States is offset by a good crop in another part, or when a government (as in France) follows the practise of lowering—not raising!—import duties when a short crop causes a deficit and threatens a high price. General economic considerations of this kind

rarely get attention in the tariff talk of the everyday man, and none at all in that of the vociferous politician. It is quite among the possibilities that in times to come the events of 1930 will be pointed to with pride as proving that the tariff saved the farmer from ruin.

Nay; it is possible that in future days the act will come to be glorified as having rescued the entire country from disaster. The present period of depression will run its course; in one year or two or three — who can say how soon? — matters will once more be moving smoothly and prosperously. Then the chronology and meaning of the events of 1929–30 will be forgotten by most people, and it will be possible to say that there was a great panic in 1929, and that the sovereign remedy was applied in the Tariff Act of 1930 and promptly brought a return of prosperity. Stranger talk is a familiar part of our tariff debates, nor is there any clear indication that it fails to serve the protectionist's turn. One hesitates to say, at least on this range of subjects, that it is not possible to fool all of the people all of the time.

As regards the powers and duties of the Tariff Commission no changes of moment were made. Virtually nothing came of the various proposals for modification. The machinery remains the same: investigation of differences in cost of production, report to the President, and "determination" of a new duty by him. The powers of the Commission are still restricted: it cannot increase or decrease rates (i.e. recommend increases or decreases) which will change them by more than 50 per cent; and it cannot remove articles from the dutiable list to the free list, or vice versa. The "bi-partisan" arrangement is retained; not more than three of the six members may belong to one political party.

Of possible moment is the new provision that the

Commission shall make investigations, not only on demands for change from persons interested, but on request of the President, and also on resolution by either house of Congress. This requirement, more explicit than anything laid down before, may have unexpected consequences. One branch of Congress (say the Senate) may load the Commission with investigations on articles where the outcome is likely to be in the direction of higher duties; while another branch (say the House) may press for investigations likely to point the other way. Congestion of business is more than possible, a need of choice between the commands, and political pressure this way and that. To keep the Commission out of politics may prove even more difficult than it was before.

The one outstanding change is in the personnel of the Commission. The terms of office of the existing incumbents are abruptly cut off, and the President is empowered to set up an entirely new body (reappointing or not as he sees fit). The salary is raised (from $7,500 to $11,000) by way of making it easier to secure men of the desired quality. The open confession of the unsatisfactory character of the body as it stood is not complimentary to the members and hardly less so to the two preceding Presidents by whom they had been selected. The step is not surprising, in view of the Commission's record; but it is taken, I cannot but feel, without proper regard to the pledges implied in the original legislation. The men had been appointed for terms of from two to six years and required to cut loose from their previous occupations. They were now turned loose with but three months' notice. As it happened, the President exercised his power of reappointment for three, and dropped the others.[1]

1. The members reappointed are Messrs. Brossard, Dennis, and Dixon. The new members are Messrs. H. P. Fletcher, a diplomat with

The hope, very explicitly and publicly stated, is that a remade Commission will not only command greater respect, and improve the details of the schedules, but will modify "inequitable" or "unjust" or "unwarranted" rates, and indeed make the whole tariff "right." Of the possibilities I need not repeat what has been said in my previous article. A chapter quite new may open in our tariff history, and indeed in the whole course of Congressional procedure. Or the result may prove to be a mere flash in the pan, with no real departure from the good old ways. Who can predict what will be the outcome?

experience on commercial matters (designated as chairman); Thomas W. Page, who was chairman of the Commission in 1919–23; and John L. Coulter, who at the time of appointment was on the staff of the Commission. The only member entirely new to its work is the chairman.

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THE RISE AND DECLINE OF ORTHODOX TARIFF PROPAGANDA

SUMMARY

Introduction, 22. — Failure of early attempts to organize for propaganda purposes, 23. — Birth of the American Protective Tariff League, 24. — Description of its activities, 25. — Dispute with Henry O. Havemeyer in 1899, 31. — Relation of the Iron and Steel industry to the League, 33. — Falling away of the League's support and subsequent decline of its activities, 34.

UNTIL within a year or two only the economist and the Democrat dared to raise voices in seemingly unpatriotic and sacrilegious opposition to the protective tariff. Most others worshipped at its shrine. Newspapers praised it, Republican politicians lauded it, and the common man, who heard it constantly associated with the words "God," "home," "native land," and "Old Glory," clung to it with a feeling that became a religious conviction. Such was tariff orthodoxy. But the recent attitude towards the Hawley-Smoot tariff bill was different. Many newspapers denounced it, prominent business men condemned it, and the people in general accepted the petition of the economists as the "collective voice of experts." Behind this reversal of public sentiment lies the story of the rise and the decline of tariff orthodoxy.

Its origin goes back to the election of James A. Garfield, whose utterances on the tariff issue during his congressional career created some apprehension among the protectionists. The appointment of Hugh McCulloch, who was regarded by many as a tariff reformer, as Secretary of the Treasury, increased this uneasiness.

Worried by the growing opposition of the public to the high tariff, the protectionists organized the "Metropolitan Industrial League" of New York City to resist the "persistent efforts of the theorists in political economy" and to combat the work of the Coblen Club, which they believed to be sending a great deal of free trade literature to the United States. They were dismayed and alarmed at the winning of the New York gubernatorial contest by Grover Cleveland and the various Democratic victories in the congressional elections of 1882. In the spring of 1883 a group of industrial leaders met at the Union League Club in New York City to discuss ways and means of popularizing protection. Out of this meeting grew the "Association for the Protection of American Industries" of which General Ulysses S. Grant was the first and only president. For a few months it was very active, but soon exhausted its energies. No attempts were made to revive it until late in the following autumn, when the defeat of the Republican presidential candidate aroused the protectionists from their lethargy.[1] Then the first step was taken by the Eastern Pig Iron Association of Pennsylvania, at its meeting in November. The following resolution was passed: "Resolved, that the president appoint a committee of five members of the Eastern Pig Iron Association of Pennsylvania to take into consideration the subject of a general organization representing the different industries of the United States in the interests of protection, and report at the next meeting." The men who served on this committee were Mr. Henry S. Eckert, president of the Eastern Pig Iron Association; Mr. William A. Ingham, president of the Rockhill Coal and Iron Works; Mr. Comly, president of the North Pennsylvania Railroad Company; Mr.

1. American Economist, May 24, 1895, p. 242.

Joseph E. Thropp, engaged in the iron and steel business at Schuylkill, Pennsylvania; and Mr. Taylor, who was connected with the iron and ore interests of New Jersey. On December 12th, the committee reported that the chief need of the manufacturers was an association that would be "aggressive to the utmost, not weakly waiting to be attacked, but pushing the fight at every point." It was then decided to call a meeting for January 15, 1885, of all persons interested in such an association.[2] Out of the meeting grew the American Protective Tariff League.[3]

It was felt by the organizers that the president should be a man not directly connected with the benefits of the tariff nor so conspicuous in political affairs "as to have enemies and hence bring adverse criticism upon the organization," yet one whose name was so widely known as to attract the attention of the public. Robert Todd Lincoln, son of Abraham Lincoln, was such a man, and he was offered the presidency of the organization. He refused to accept and Mr. Edward H. Ammidown of New York City was then selected.[4] The current expenses of the organization were to be met by dues collected from its members, who were to be known as "Defenders of American Labor and Industry." Each person or firm upon becoming a "Defender" signed a pledge to pay annually to the organization "such portion of one hundred dollars as may be asked in any year

2. Ibid.

3. On that day the following prominent business men assembled at the Astor House in New York City: William L. Strong, Person C. Cheney, Calvin Wells, Edward H. Ammidown, Joseph E. Thropp, Cornelius N. Bliss, LeGrand B. Cannon, Theodore C. Search, Thomas Dolan, George E. Ely, Smith M. Weed, George Draper, William A. Ingham, Henry B. Metcalf, William P. Sinn, Arthur Soper, E. A. Hartshorn, Henry Cook, Oliver Williams, Charles H. Camp, Levi L. Brown, Robert P. Porter, and Joseph D. Weeks.

4. Ibid., January 23, 1891, p. 59.

by the executive committee." [5] At the end of every year the expenses which had not otherwise been provided for were divided among the members. According to its constitution the League was organized for "the protection of American Labor and Industry" but in reality it existed to oppose any reduction in a high tariff law.[6] Until 1926 it never deviated from this course by admitting the advisability of the lowering of any duty. Under a high tariff régime, it was the personifier of the stand-pat idea; but under a low tariff régime, it was an aggressive agitator for tariff reform.

The League began the publication of a monthly Tariff League Bulletin in 1887. Two years later the name was changed to the American Economist, under which name the organ was published weekly until July, 1926. Then a more dignified monthly magazine, the Tariff Review, was substituted. The circulation of the American Economist was 14,000, but through its masterful system of newspaper syndicates the editors claimed to be reaching 24,000,000 readers a week.[7] Every Monday they sent out two columns of editorials, news articles, high-tariff arguments, and appropriate cartoons for publication in 141 of the larger daily newspapers, and every third Monday they supplied a full page of reading matter on the tariff issue to a nation-wide press syndicate through whom the service was furnished to local editors.[8] In addition, copies of the American Economist were sent regularly to 5,000 newspapers, who were at liberty to reprint any of its articles or cartoons.[9] One

5. American Economist, October 6, 1893, p. 212.

6. The American Economist once described its policy as opposing "the crossing of a *t*, the dotting of an *i*, or the changing of a punctuation point in the existing tariff law." May 1, 1903, p. 205.

7. American Economist, May 1, 1903, p. 203.

8. Ibid., January 27, 1905, p. 37.

9. In 1918, 6316 newspapers were receiving the American Economist.

may never have seen a copy of the American Economist, nor have been aware that it existed, yet may have read and absorbed its propaganda daily. Beside these activities, the League published every year a number of pamphlets which emphasized some single feature of the tariff issue. For example, during 1904 it distributed to the business men of Iowa, Minnesota, and New England 90,000 copies of a document entitled "Impossible Reciprocity" which had been specially written to combat the agitation in those localities for free trade in natural products with Canada.[1] In this year it claimed to have sent out 39,307,815 pages of such "standard documents," in addition to printing and distributing 203,250,000 pages of presidential campaign material for the Republican National Committee.[2]

In the first issue of the American Economist the editor entered into a dispute with Frank W. Taussig, then a young professor at Harvard. Altho other opponents had come and gone with the years, Professor Taussig seemed to stay on forever, so much so that in the first issue of the Tariff Review the editor called attention to the fact that for 40 years Professor Taussig had proved to be the League's most formidable enemy.[3] This constant wrangle with the "halls of learning" early suggested to the American Economist that a large part of its opposition originated in the free trade teachings of the colleges. In 1888 the League made an unsuccessful attempt to introduce the theory of protection into the college classroom by the publication of a voluminous treatise on political economy entitled Principles of Economic Philosophy. This book had been written under the auspices of the League by Van Buren Denslow for

1. Ibid., p. 44.
2. Ibid., p. 46.
3. Tariff Review, January, 1927, p. 8.

the express purpose of ending forever the claim of college professors that a suitable textbook in economics advocating protection did not exist.[4] Throughout its pages the advantages of home production over foreign trade were stressed; for example, "In the purchase of foreign iron, only the price we pay represents American labor, while in the purchase of American iron both the price we pay and the thing we pay it for represent and are the fruits of American labor. Supposing that in both cases we get the worth of our corn in iron, in the case of the imported iron we give employment only to the labor that produced the corn, while in the case of the American iron, we give employment to the same amount of American labor in producing the corn and an equal amount in addition in producing the iron." [5] A few years later the League sent questionnaires to the seniors of the leading colleges requesting information concerning the attitude and teachings of their professors in political economy. The replies confirmed the fact that the majority of college teachers favored free trade. To counteract this influence, each fall the League secured the names of those students who would graduate the following June and forwarded to them "literature of the type that would appeal to intelligent minds." The style of this "literature" is indicated by the following titles: American Tariffs from Plymouth Rock to McKinley, The Glory of Protection, and Protection is Panic Proof. For a number of years its requests for names did not meet with the approval of the college authorities, but it was able nevertheless to secure yearly the addresses of 24,000 graduates. Also, it began the practice of sending the American Economist and its other publications to the libraries of the colleges and

4. American Economist, October 12, 1888, p. 170.
5. Denslow, Principles of Economic Philosophy, chap. 14.

universities throughout the United States.[6] By 1918 there were over 500 schools on its mailing list.

Just preceding the Roosevelt-Parker contest of 1904, the League launched its first "new voters campaign." This idea had been suggested by Mr. Theodore M. Ives and had met the "approval of every earnest protectionist to whom it was submitted, including the President of the United States." From its subscribers and correspondents and from Republican politicians, the League obtained the names of over 200,000 persons who would vote for the first time in the coming election and mailed to each seven or more of its propaganda pamphlets. After the election the American Economist boasted that "in not a single congressional district of a doubtful state, except Maryland," into which this material had been mailed, "did protection fail to obtain a plurality." [7]

The American Protective Tariff League had two other modes of disseminating propaganda: its 2,000 "official correspondents" who kept it informed of the needs of each locality and aided in the distribution of its documents,[8] and its "Commercial Travelers' Bureau," composed of 10,000 or more traveling salesmen, organized to spread the tariff gospel by word of mouth in their numerous contacts with the public in hotels, offices, and on trains.[9]

In its preparation for the McKinley campaign of 1900, the American Protective Tariff League sent questionnaires to business men inquiring about industrial conditions in March 1899 as compared with March 1895. The 1900 replies showed a 39.56 per cent increase in the number of men employed, a 54.09 per cent increase in the total monthly payroll, and a 10.49 per cent increase

6. American Economist, January 27, 1905, p. 44.
7. Ibid.
8. Ibid., May 24, 1895, p. 249.
9. Ibid., January 28, 1921, p. 32.

in the average workman's wages. Assuming the Dingley Tariff Act to be responsible for the improvement, the American Economist wrote "such is the story of Protection and Prosperity as affecting the American wage earner." [1] This "Industrial Census," as it was called, appeared in five consecutive issues of the American Economist, was distributed to its syndicate, to its press association, and to the 5,000 newspapers on its mailing list, and finally, was put into documentary form for special distribution to voters, libraries, and college students.

To win western support for the McKinley Tariff Act, the American Economist began agitation in 1889 for a duty on hides. At that time the price of livestock was 50 per cent lower than it had been in 1883. The cattlemen were complaining and a Senate committee was investigating the packers. Here existed a situation ready made for the American Protective Tariff League. All that was needed was to distribute the propaganda. It had not occurred to the cattlemen themselves to seek aid through a tariff duty. During the summer of 1889 a Mr. Garland, writing to the American Economist about conditions as he found them during a trip through the Dakotas and Montana, said, "As this is a land of grass, the pastoral interests predominate. Sheep and cattle in large herds and horses in smaller numbers are seen on all sides." He added "the wool growers are protectionists" but did not mention the cattlemen as leaning towards protection even tho "the low price of cattle brings the profit from raising them to nil." [2] He believed that the keen competition of foreign hides would eventually force the cattlemen into the protectionists' ranks. The interesting point is that, after a year of hard

1. American Economist, October 20, 1899, p. 184.
2. Ibid., September 20, 1889, p. 188.

times, it had not occurred to the cattlemen to turn to protection for a solution to their problem. Spurred by the necessity of holding western senatorial votes, the protectionists issued a pamphlet "especially interesting to farmers and stockraisers." [3] The purpose was to educate the ranchmen to the value of protection. According to this document "it is unquestionably true that the condition of the stock interests is directly attributable to the Democratic free trade policy. . . . You will ask how does free trade affect this result. Please notice the statement of hides imported from 1860 to 1889 which shows the enormous amount received into this country each year." After a table giving the imports, there followed, "Every dollar of this vast amount comes into direct competition with you." Then came the appeal: "Hides are to the tanner what wool is to the wool manufacturer. Wool is and should be protected. Hides are to the tanner what iron ore is to the smelter. Ore is and should be protected. . . . To secure this protection every person directly or indirectly interested should see to it that a Republican Senate is assured through your legislatures. See to it that the small majority in the House of Representatives and in the United States Senate is increased, and the interests of your new State will have every consideration and advancement." This high pressure propaganda was continued intermittently by the American Protective Tariff League through the pages of the American Economist until after the congressional elections of 1890. Then the matter was dropped, and a duty on hides was not mentioned again for years.

The relation of the sugar industry and the American Protective Tariff League is of interest. The United

3. Ibid., September 6, 1889, p. 154. The title page read: How the Stock Growing Interests Will be Benefited: A Practical Illustration of Protection to American Industries, by W. F. Wakeman, Kansas City, Mo. (by the authority of the Republican National Committee).

States consumes from three to four times the amount of sugar which it produces. Most of the imported supply comes from Cuba and is largely refined by the American Sugar Refineries Company. The sugar industry is divided on the question of the tariff into two camps: on one side the American sugar refiners who want free trade in raw sugar, and on the opposite side the beet sugar producers who ask for protection. As would be expected, the Cuban sugar interests have denounced the tariff policy while the beet sugar interests have praised the American system of protecting American industries. In this clash the American Economist has been the mouthpiece for the beet sugar interests.

On June 14, 1899, Mr. Henry O. Havemeyer, president of the American Sugar Refineries Company, seized the opportunity presented him by the Senate investigation of trusts to strike a blow at the tariff. During one of its hearings, he stated, "The mother of all trusts is the custom tariff." This much-quoted statement threw the editors of the American Economist into a furore and they devoted column after column to its refutation. They called attention to the fact that the Sugar Trust was organized during Cleveland's "demoralized low tariff term" [4] and that the largest of all trusts, the Standard Oil Company, had never enjoyed protection. They described Havemeyer as a "malevolent old sorehead" who was trying to destroy all protection because the tariff on refined sugar was not as high as he wished it. They pointed out that in "free trade England" trusts were regarded "as a matter of course" [5] and claimed that "the real mother of trusts was British free trade." [6] Protection was the "savior of our industries, the herald of advancing wages and the progenitor of

4. American Economist, June 23, 1899, p. 295.
5. Ibid., July 21, 1899, p. 26. 6. Ibid., August 18, 1899, p. 57.

good prices." [7] A few years later the United States government arranged a reciprocity treaty granting Cuba a 20 per cent reduction in the sugar duty. This measure, one phase of our general policy toward Cuba, was attacked by the American Economist as a favor granted to the American Sugar Refineries Company at the expense of American producers. Subsequent investments of the American Sugar Refineries Company in western beet sugar factories encouraged the editors of the American Economist to predict Havemeyer's early conversion to the cause of protection,[8] a prophecy which failed, for the tariff fight of 1913 found the Cuban sugar interests still agitating for free trade in unrefined sugar.

Altho home producers of sugar all stand to gain from a high tariff, only a part of them have been willing to support the American Protective Tariff League. The beet sugar producers of the West, excluding California, have never been contributors to it. The League receives more support from Michigan and California than it does from all the rest of the sugar beet states together.[9] For example, in 1923 these two states supplied 35 "Defenders" to the League and produced 24 per cent of the beet sugar of the United States, 94 per cent of it being produced at a loss; while the remainder of the beet sugar states supplied 13 "Defenders" and produced 76 per cent of the total with only 14 per cent produced at a loss.[1] This would seem to indicate that the support of the American Protective Tariff League was drawn from the high cost producers within the protected industries.

7. Ibid., July 21, 1899, p. 27.
8. Ibid., January 27, 1903, p. 46.
9. Yearly report of the League's "Defender" roll, published each year in a January issue of the American Economist.
1. Tariff Commission Report on Sugar, 1926. The figures are to be taken with a grain of doubt, but they indicate the difference between the two groups.

The League has always complained of the lack of support given it by the Louisiana sugar growers, who gain also from the duty on sugar.[2]

The relation of the iron and steel industry to the American Protective Tariff League presents an interesting episode in the history of tariff orthodoxy. This industry, which had taken the lead in the organization of the American Protective Tariff League in 1885, was the first to desert it and enter into active opposition to it. The great boom in the production of iron and steel in this country came shortly after the Civil War when, protected from foreign competition by the tariff wall, the industry was slowly gaining the home markets. In need of protection, it supported the American Protective Tariff League. In 1897 the American Economist called attention to the fact that the United States was beginning to export steel and pointed out that this was the result of protection granted to an industry in its infant stage.[3] To those who suggested that the duty on steel could then be abolished it replied that, since the price was less here than abroad, no harm resulted from letting it alone. However, as the steel industry became more and more interested in its foreign markets, it began to withdraw its support from the American Protective Tariff League and to talk of reciprocity arrangements with foreign nations, which brought it into direct opposition to the work of the League. At the Washington Convention of the National Association of Manufacturers in 1902 one of the speakers said, "We know that some of you people would be hurt by lower tariffs but you would not be hurt as much as we would be helped."[4] The fight for reciprocity once started, the American

2. American Economist, May 1, 1903, p. 205.
3. Ibid., October 15, 1897, p. 198.
4. Ibid., April 25, 1902, p. 197.

Economist fought it to the end. It openly boasted that it had forced the President of the United States to cease his agitation for tariff reform by threatening to defeat him for reëlection in 1904.[5]

Hardly had the League regained its breath from this fight when it was forced to face a hostile attack from a hitherto staunch friend and supporter, Andrew Carnegie, who stated, in an article published by the Independent,[6] that iron and steel needed no protection and that after any industry had enjoyed it for a reasonable time, the protection should be withdrawn and the industry left to shift for itself. The American Economist attempted to belittle the importance of this statement by calling Carnegie "a senile old man," and "an ingrate" who was no longer engaged in the steel business but drew his revenues from bonds and whose opinion therefore should not weigh heavily on the subject.[7] This contrasted markedly with a statement in an earlier issue of the American Economist which strove to prove the infallibility of his judgment. On that occasion Carnegie had defended a duty on steel and a Mr. White had protested as to Carnegie's qualifications as an authority since he was interested financially. "On the contrary," replied the American Economist, "he is thereby the better qualified to give out intelligent information as to the needs of industry." [8]

In 1926 the American Protective Tariff League altered its plans and policies to adjust them more adequately to changed economic conditions. Improvements in methods of journalism and advances in the general level of public intelligence made it advisable to substi-

5. Ibid., May 1, 1903, p. 203.
6. December 31, 1908, p. 1628.
7. American Economist, December, 1908, to June, 1909.
8. Ibid., October 31, 1890, p. 274.

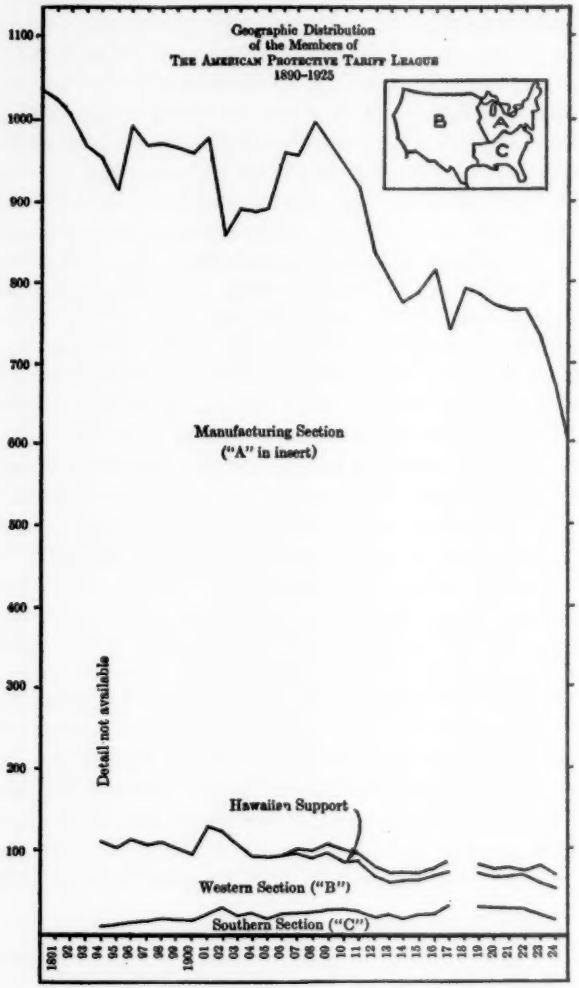
tute the Tariff Review for the publication of the American Economist. Altho the American Economist had always had a wide gratuitous distribution, it was thought best to make a charge of three dollars a year for the Tariff Review. This was later reduced to one dollar. The League's policy of opposing every suggestion for tariff revision was modified so as to permit the pages of the Tariff Review to be open to "sincere and informed tariff revision proposals," altho notice was served that no "attacks on the present American policy of an adequate tariff" would be allowed to go "unchallenged or unanswered." [9] The new publication was to pay no attention "to the academic arguments advanced by thorough-going free traders." It was to proceed on the theory "that the last word on the principles of free trade was adequately said one hundred and fifty years ago by the founder of modern political economy, Adam Smith." It was to assume that free trade was dead since "the most noted modern economists now admit that a protective tariff is essential to many industries." [1] The future battle would be on the "degrees of protection." Perhaps the League hoped that by repeating this with sufficient assurance it would come to be accepted as a fact by the majority of people.

The mere window-dressing of its ideals, however, did not solve its major difficulties. That 80 to 90 per cent of the League's support came from the manufacturing section of the United States is shown on the accompanying graph.[2] In late years this support has fallen away rapidly. The total number of "Defenders" dwindled from over a thousand in 1890 to less than six hundred in

9. Tariff Review, January, 1927, p. 8.

1. Ibid., p. 3.

2. The totals used in the graph are computed from the League's "Defender" roll published each year in a January issue of the American Economist.


Geographic Distribution
of the Members of
THE AMERICAN PROTECTIVE TARIFF LEAGUE
1890–1925
A
B
C
1100
1000
900
800
700
600
500
400
300
200
100
Manufacturing Section
("A" in insert)
Detail not available
Hawaiian Support
Western Section ("B")
Southern Section ("C")
1891
92
93
94
95
96
97
98
99
1900
01
02
03
04
05
06
07
08
09
10
11
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1925. The graph fails to depict the actual loss, however, for of the 598 members remaining in the latter year, only 287 paid their assessment.[3] This withdrawal of support is the natural result of the growth of American industries beyond the need for protection. At the time of the League's organization they had been small and had been hard pressed by foreign competition. In the years that followed they had gradually gained control of the home market and by 1897 were planning invasions into foreign fields. Along with this movement came the growth of monopolies and trusts. Protection was unnecessary to these more powerful combinations.[4] The American Protective Tariff League had always drawn its support from the smaller companies. If these merged into larger organizations "they invariably ceased to contribute to the League."[5] For example, before the promotion of the United States Steel Corporation there were 110 members in the steel industry, contributing $11,000 a year, but by 1906 only one of these members was left in the ranks. The editor of the American Economist wrote, "I have in mind another large corporation which formerly by its subsidiary companies carried seventeen memberships. From this organization we do not receive a contribution of any character. As a matter of fact, the evolution of independent concerns into large corporations has the practical effect of reducing our revenues so as to seriously impair the usefulness of our organization."[6] The changed economic status of the country in the last decade has made the tariff policy of the League impractical. It has wanted the home market exclusively for domestic producers; it

3. American Economist, January 29, 1926, p. 37.
4. Ibid., May 1, 1903, p. 207.
5. Ibid., p. 203.
6. Ibid., September 14, 1906, p. 125.

has demanded that exports exceed imports; and it has opposed the cancellation of the war debts. The contradictory nature of these desires, together with the development of American industry, has made the League's path arduous. The American Protective Tariff League recognizes these difficulties but points to a different set of causes. Mr. W. Warren Balbour, president of the League, in an article entitled "Protective Tariff on the Defense," cites six causes for its troubles.[7] "The first," he writes, "is the far too prevalent attitude of complacent indifference on the part of American producers towards their dependence upon the tariff." Only a "baptism of fire," he believes, "such as the country had in 1894 can convert them." The second is the agricultural situation which is a strong anti-tariff factor. "It seems to avail little to point out to a certain group of farmers in this country that the majority of their crops are protected under the Act of 1922." The third is a group of internationally minded persons including two types, the financiers and the industrialists. The first advocate free trade as a method of collecting the war debts and the second are drawn to it because of their desire to build up a large export trade. The fourth cause is the American women, who have a prejudice against the tariff because it raises the prices of things which they purchase. Formerly this opposition did not count for much but since the nineteenth amendment it must be considered. The fifth is the increased attitude of aggression on the part of foreign countries towards the tariff policy, for an example of which he cites the recent French controversy. The last but most important in his mind is the fact that less than half of the Representatives and Senators who helped pass the Act of 1922 still retain their seats in the present Congress.

7. Tariff Review, January, 1928, p. 5.

"These six forces," he concludes, "stand out as the dominant factors in the tariff situation today and account for the fact that the American policy of a protective tariff is on the defensive. Only the strongest line of opposition to these attacks on the American tariff policy will save the situation." Such is the official testimony to the decline of tariff orthodoxy.

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EQUILIBRIUM ECONOMICS AND BUSINESS-CYCLE THEORY: A COMMENTARY

SUMMARY

I. Dr. Kuznets' methodological scepticism, 40. — II. Lederer, Loewe, and Carrel on equilibrium economics; the empiricorealist as circular reasoner, 42. — III. The equilibrium concept and economic dynamics, 55. — IV. Rosenstein-Rodan "discovers" orthodox economic theory, 63. — V. Business cycles and the Laws of Chance, 67. — VI. The program of Empirico-Realism provides neither a synthesis nor a call to economic theorists to abandon their evil courses, 81. — VII. The real problem of economic dynamics, 86.

I

Dr. S. Kuznets' article under the above title in the May issue of this Journal seems calculated to arouse mixed feelings in the minds of other earnest grapplers with the thorny problem of how to advance the development of "economic dynamics" to a stage that will render it a more helpful partner in the task of selecting, collecting, arranging, and analysing statistical data of the time-series type, with a view, in particular, to achieving a more adequate understanding of the business cycle, and a better scientific basis for organised social control.

Some of these, belonging as they do to "the current generation of economists," who "have been nurtured on the equilibrium concept," are, as Dr. Kuznets rightly suspects, "trying to build up," if not anything quite so ambitious as "a new economic system," at least their own small contribution to a more adequate dynamic theory, "by starting with the concepts they were taught, and introducing the necessary complications to

secure a better fit to reality." They are, of course, irretrievably bound by the limitations of their generation and upbringing, and if this "rôle" has been thrust upon them "as a matter of historical development" it would doubtless be futile for them to attempt to evade it. Nevertheless, it will perhaps not offend the canons of propriety for one of them to venture to discuss (quite in the abstract) the doubt which Dr. Kuznets — viewing the problem *sub specie aeternitatis* — entertains as to whether "methodologically [this is] the advisable road to follow."

In embarking on this somewhat adventurous project, one is disconcerted at the outset by Dr. Kuznets' bold opposition between "economic theory" and "business-cycle theory." Almost any member of the "current generation of economists" would probably admit, in all humility, that "general economic theory" requires modification and extension in order successfully to "incorporate" the facts disclosed by the recent and current intensive empirical studies of business cycle phenomena. In so doing he would be but recognising the "organic relation between business-cycle theory and theoretical economics," the perception of which by Böhm-Bawerk in 1898 is approvingly noted by Dr. Kuznets. Indeed, he would probably hold that the formulation of a fully developed "business-cycle theory" would itself involve the construction of an adequate "economic dynamics." If, therefore, such a fully developed business cycle theory already exists, it would seem (on this view of an organic relationship between the two) that the problem of constructing an economic dynamics has already been solved — at least in its main outlines, and to such an extent as to render methodological discussion unnecessary. If not, then presumably business cycle theory, like general economic

theory, is susceptible of further development, and the methodological issue raised by Dr. Kuznets is worthy of investigation.

II

Three successive champions are advanced by Dr. Kuznets in his attack upon the validity (or utility, or practical relevance) of equilibrium economics. The first is Lederer. "According to Lederer, pure economic theory cannot explain dynamic phenomena satisfactorily, because its static laws of value and price fail to take into consideration the type of social economy, the interrelations of different groups of economic agents. Pure theory . . . must therefore be supplemented by another approach. 'Such an approach would strive to clarify a particular form of economic system. It would start from the circulation process of the typical economy, describe it and establish its conditions. It would then present the totality of economic phenomena, throw light on their movements and establish their social conditions. Each of these economic systems has its social side, whose establishment is necessary for the presentation of the economy as a social process.'" (Page 386.)

Large tho the aspirations of Lederer obviously are, lucidity does not appear to be one of his leading characteristics. A pure theory whose only way of "explaining . . . processes and factors of change" is to treat them as "deviations from its static laws" can hardly be said to have explained them at all: they are for it essentially unexplained residues. If its approach were indeed such that this is all that it *could* do, then undoubtedly it would require to be supplemented by another approach. But the question as to whether general economic theory *is* thus methodologically handicapped is precisely the point at issue.

The use of the term "pure" is an unfailing cause of confusion and misunderstanding. Presumably the only really pure (or formal) sciences are formal logic and pure mathematics. So-called pure economics must either be indistinguishable from pure mathematics, or must, in fact, take for assumptions certain "material" elements, even if these be only scarcity, economic "rationality," and the satiability of particular wants. The term "pure economics" has in fact been employed to designate a theoretical system which bases itself upon those material elements only that are regarded (rightly or wrongly) as universal and inevitable in all possible types of human economy, and which works out the general implications of the mutual interaction of these elements. Cassel's Theory of Social Economy explicitly emphasises the importance of thus exhibiting the essential nature of all economies, and then progressively introducing more and more material elements selected as relevant to the particular economy in which we are interested. If Lederer has shown that the introduction of material elements not common to all economies in some way necessitates a revolution in method, Dr. Kuznets has not succeeded in making this clear. But in fact, as he himself points out, "Lederer does not specify what should be the relation of pure theory to this new theoretical body. He therefore is unable to point more definitely to the logical peculiarity of the pure theoretical system which does not permit the inclusion of social conditions."

It might well seem at this stage of the discussion that the real point at issue is not the number and kind of material elements (social conditions), as such, that are incorporated in the theory, but the question as to the relation of "statics" to "dynamics." However, it emerges later that for Dr. Kuznets the real reason why

"social conditions" are incapable of being incorporated in pure theory is that it is they that are responsible for the various "time spans" required for the adjustments necessitated by the existence of "dynamic" conditions. For the present writer, on the contrary, it is precisely the translation of concrete social reactions into temporal magnitudes that renders possible the development of a theoretical economic dynamics. Even such elementary factors as scarcity, economic rationality, and the satiability of particular wants, are presumably "social conditions," and there is no more reason for the assumption that these interact instantaneously in a timeless, frictionless vacuum than for assuming that more complex "social conditions" do so. To be consistent, Dr. Kuznets should eliminate even the former from his system of "pure theory." Were he to do so, he would find that everything characteristic of "economic science" had slipped through his fingers, and that he was left clutching only at the pale entities of pure mathematics.

Dr. Kuznets' second champion is A. Loewe, who delivers himself as follows:

> All the systems of economics since the Physiocrats have put in the centre the concept of equilibrium. . . . This concept of equilibrium is logically bound up with the concept of a closed interdependent system. Since it is only through independence from outside influences and through the functional interconnection of the elements of the system that the persistence of any state, i.e., of an equilibrium, is achieved. *We can thus say that any system of economics which operates with the concept of equilibrium must necessarily be a closed interdependent system — in short, a static system.* As such the theory of movement characteristic of this system is the movement by the method of variations.[1]

Dr. Kuznets adds in explanation: "By movements according to the method of variations Loewe understands changes from an equilibrium position which tend

1. Page 387. Italics mine.

to be checked *immediately* by the forces which make for the restoration of the status quo ante. These movements are, then, passing deviations, temporary variations around the equilibrium."

If I am not mistaken, the crucial statement for Dr. Kuznets is that "any system of economics which operates with the concept of equilibrium must necessarily be . . . a static system." There appears to be no reason why anyone should accept this bald assertion. In its context, it is an obvious *non sequitur*. Presumably no economist (or at any rate no economist not under medical restraint) has ever imagined himself to be living in a static world. The use of the concept of equilibrium by "all the systems of economics since the Physiocrats" therefore suggests that this concept has been found, or at least persistently imagined to be, useful as a conceptual tool in the explanation of "dynamic" economic life. Why a "dynamic" system of economics should not "operate with" the concept of static equilibrium, if it finds it useful so to do, is not in any way explained. Yet it is precisely the impossibility, or at any rate the methodological undesirability, of this that Dr. Kuznets has set out to expound.

Here again, also, the "theory of movement" characterised as "movement by the method of variations" turns out to be not a theory at all. In a stationary equilibrium exhibiting "independence from outside influences" ("outside" the "equilibrium"?) *any* movement would be inexplicable and contradictory. If changes from an equilibrium position occur, they *must*, by hypothesis, be the result of "outside influences"—not necessarily "outside the economic system" (a phrase the true meaning of which it requires some care to determine), but "outside" the equilibrium. It is precisely to such changes — changes in demand,

changes in technology, changes in the amenability of Nature to human exploitation, that "all the systems of economics since the Physiocrats" have attributed the "dynamic" character of actual economic life. The function of the concept of equilibrium has been to show that, given such changes in the conditions underlying any part of an equilibrated system, all the various elements in that system would, by virtue of their mutual interdependence, be set in motion, and would not come to rest again until, or unless, a new general equilibrium was achieved.

Dr. Kuznets' explanatory suggestion that equilibrium tends to be restored "immediately" is no part of modern general economic theory. By way of illustration, one may quote the following:[2]

> Of course the periods required to adapt the several factors of production to the demand may be very different; the number of skilled compositors, for instance, cannot be increased nearly as fast as the supply of type and printing-presses. And this cause alone would prevent any rigid division being made between long and short periods. But in fact a theoretically perfect long period must give time enough to enable not only the factors of production of the commodity to be adjusted to the demand, but also the factors of production of those factors of production to be adjusted and so on; *and this, when carried to its logical consequences, will be found to involve the supposition of a stationary state of industry*, in which the requirements of a future age can be anticipated an indefinite time beforehand. Some such assumption is indeed unconsciously implied in many popular renderings of Ricardo's theory of value, if not in his own versions of it; and it is to this cause more than any other that we must attribute that simplicity and sharpness of outline, from which the economic doctrines in fashion in the first half of this century derived some of their seductive charm, as well as most of whatever tendency they may have had to lead to false practical conclusions.

This passage not only effectually disposes of the claim that modern general economic theory — at any rate "of the Marshallian type" — visualises an economic

2. Marshall, Principles, p. 379, n. Italics mine.

equilibrium disturbances of which are checked immediately; it also brings out clearly the point which Dr. Kuznets, invoking Rosenstein-Rodan for the purpose, develops later in his article: namely, that under actual dynamic conditions long-period equilibrium (and particularly the general equilibrium of the stationary state) is, owing to the inequality of time-reactions combined with the constant emergence of new stimuli to further readjustment, *never* reached.

Loewe, we are told, then "goes on to analyse the current business-cycle theories to see whether the solutions which they offer are within the bounds of the theoretical system of economics."

All those which "rely on error or refer to the anarchy of production" are found "obviously to deny the most basic principle underlying all general economic theories," because they "assume that the business man will not behave rationally, that his behavior does not follow the same rules which it is supposed to follow in the theoretical system of economics." As examples of such theories, Dr. Kuznets cites "those of Hardy or of Pigou." He later inquires: "Can one say that Pigou's theory of industrial fluctuations assumes the same kind of rational economic behavior as is assumed in the general Marshallian theory of the equilibrium of demand and supply?"

Two points may be briefly made.

In the first place, Pigou's theory of industrial fluctuations is a complex one: it attempts to give due weight to each of three general types of causes — "real," psychological, and monetary. It is presumably to the second of these that reference is meant; i.e. to "errors of optimism and pessimism." Now, according to Marshall,[3] "in the present book normal action is taken to

3. Principles, Preface to the First Edition, p. vi.

be that which may be expected, under certain conditions, from the members of an industrial group." If Pigou is right (and this is not in question) in holding that, "under certain conditions," we repeatedly find "the members of an industrial group" (or groups) exhibiting mass pessimism or mass optimism, according to circumstances, — so repeatedly, indeed, as to justify us in embodying the fact in our explanation of business cycles in general,—then there is surely no inconsistency with Marshall's definition of the term "normal" in describing such optimism or pessimism as the *normal* response of the groups concerned, under the given conditions. Indeed, if through a widespread improvement in the economic education of business men (or from any other cause) this optimism and pessimism were to be eliminated (or even to some extent reversed), we should then be correct in describing the new set of responses as "normal" under the given conditions — provided, of course, they became sufficiently permanent.

In the second place, the use of the term "rational" is ambiguous. One suspects that Dr. Kuznets would regard it as implying some form of psychological or ethical "hedonism." But neither Marshall [4] nor many of "the current generation of economists" find any such implication necessary. The essence of (psychological) hedonism is that "pleasure" is the aim of all effort. We do not need to assume that "rational" choice is necessarily a choice among "pleasures": it may be among objects, desires, wants, or what you will. But further, we have no need to assume, nor does Marshall assume, that all choice is "rational" even in the sense of being "deliberate, and the outcome of cal-

4. Principles, p. 17, n., and pp. 92–93.

culation." [5] Given a limited income and a multiplicity of wants, *some* sort of choice is inevitable. Given the same income, and a rise in the price of one or more of the things on which it is spent, and *some* readjustment of expenditures is again inevitable.[6] This is not a matter of interpreting current fashions in psychology: it is a matter of simple arithmetic. The bare bones of general economic theory therefore remain intact. What is rendered dubious and difficult is the interpretation of the social and philosophical significance of the findings of this theory, in the light of social psychology, sociology, and moral and political philosophy. It is the optimism of Bastiat that is out of date.

5. Marshall, Principles, p. 20. Altho "the side of life with which economics is specially concerned is that in which man's conduct is most deliberate, and in which he most often reckons up the advantages and disadvantages of any particular action before he enters on it."

It may be suspected that those who credit "modern psychology" with having undermined "general economic theory" do not always trouble to distinguish the various possible meanings of the term "rational." We may agree whole-heartedly with the innuendoes of such social satirists as Veblen, for example, that the actual objects or "satisfactions" at which men and women in fact aim are "irrational" in the light of a valid philosophical standard of human values, and yet hold that, given a human being equipped with these (philosophically) "irrational" values, it is possible for him to pursue them with the highest degree of (economic) "rationality" — calculating to a nicety how he may derive from his limited income a maximum of "satisfaction" for his morbid tastes.

Again, we may demonstrate that for any one member of a society of "economic men" the task of *objectively* "rational" calculation of the course conducive to his maximum self-interest is impossible: (*a*) because he has not all the necessary data at his command, (*b*) because, even if he had, the resources of mathematics would be unequal to the necessary calculations; and yet recognise that, in the light of his available knowledge and of the practical possibilities of calculation, he may "rationally" seek to "do the best he can for himself."

But, for the purposes of general economic theory, the assumption of the universality of even this order of "rationality" is not necessary.

6. Emphasis upon the pervasiveness and importance of social and class custom and habit would seem to lead to the hypothesis of greater rather than less stability and definitiveness in the character of such responses (on a mass or group scale, which is all that concerns the economist) under given conditions and over a "reasonable" period of time.

The second group of business cycle theories is stigmatised by Loewe as "the group of circular reasoning," because they "begin by assuming a state of depression or a state of prosperity . . . But the explanation is implied in the assumption."

Apparently it is here the theories that are condemned rather than equilibrium economics. But, in any case, I confess that I am unable to follow this reasoning. To the uninitiated it would seem that an economist who began by analysing a state of depression and ended by showing, not only why this state could not continue indefinitely, but also that it must give place to successive phases, passing through prosperity and returning again to depression, had accomplished something more than a piece of "circular reasoning." If it be indeed true that the explanation is implied in the assumption, then the only unsolved problem is whether or not the economic system ever was in the past in a state of perfect equilibrium, and, if so, how it got out of it — a problem which might be turned over to the economic historians.

The third group of theories is called "the group of generalising theories," or all those "which generalise a partial disturbance into a general disturbance . . . . From the point of view of equilibrium economics, a partial disturbance should be cancelled by an opposite change in some other part of the system."

This assertion has already been refuted in dealing with the claim that, according to modern general economic theory, equilibrium tends to be restored "immediately." It is effectively disposed of by the quotation from Marshall given above.

The fourth group of theories are those of "time discrepancy." "The assumption of interdependence of the elements in the closed system imposes a limitation.

Since each of the elements is equally dependent upon all the others, the time-span which elapses from the start of a certain change (or disturbance) to its working out in all the elements of the system must be *equal* for each and all the elements . . . . The assumption of varying time-spans for the reaction of the separate elements destroys the general interdependence."

Here again I am compelled to confess that I do not see why one should accept the *ipse dixit* of Loewe for this extraordinary statement. To revert to the quotation from Marshall, one is asked to believe that because the time-spans occupied respectively by an increase in the demand for printed matter, an increase in the supply of type and printing-presses, and an increase in the supply of skilled compositors are admittedly very different, therefore one must give up the cherished conviction that these things are interdependent! One might be excused for lapsing into a current misuse of language and attaching the popular epithet "metaphysical" to these cryptic utterances. Perhaps, however, they are only another instance of "circular reasoning."

The fifth and last group of business cycle theories are "the theories of independent variables," or those "which in their explanation of business cycles rely on factors outside the economic system, such as climate, crop, technical progress. All these theories are obviously non-economic theories of business cycles . . . . To interpret business cycles in this way is to confess the failure of economic science to explain their appearance."

The important question here would seem to be whether, as a matter of actual fact, these "factors outside the economic system" are, or are not, the basic causes of business cycles. If they are, and if explanation in terms of them falls outside the confines of economic science, the best thing economic science can do

is to confess its failure gracefully. The matter, however, is not quite so simple, and there appears to be no reason why economic science should fear for its prestige on this account. In the first place, when the combined interaction of A with B produces certain consequences, an adequate explanation of the causes of those consequences requires analysis of the characteristics of both A and B. If business cycles are in fact "due to" climate, or crops, or technical progress, they are still the result of the impingement, as it were, of these things on the economic system; and the fact that the latter responds to this impingement by wave-like undulations throughout its whole structure is scarcely adequately explained by citing "climate," or "crops," or "technical progress." It still falls to the economist to explain just why his system should behave in this way under this kind of treatment from "outside."

But, secondly, the phrase "outside the economic system" (an inevitable pitfall for the unwary who have not pondered Marshall's "philosophical conception of society as an organism") is obviously incorrectly employed. If not only peculiarities of climate and crop-yields, but also technical progress, is to be regarded as outside the economic system, there appears to be no reason why all changes in the underlying conditions of supply should not be similarly ruled out. And why exempt changes in demand? By the time the business cycle theorist has eliminated all these elements making for dynamic change from his economic system, he will bid fair to be in the same plight as Dr. Kuznets' equilibrium economist — forever doomed to seek vainly for some explanation of change within a realm from which all factors of change have been ruthlessly excluded at the outset. To appreciate the significance of the *organic* view of the economic system is to understand

(a) that incentives to growth or change may arise within the organism as well as in the environment, and (b) that so-called dynamic adjustment is, in the sphere of organic life, merely an abstract phase of concrete organic adaptation.

It is not possible, then, to agree with Dr. Kuznets as to the "obvious" bearing of Loewe's analysis: that all business cycle theories either "attribute the fluctuations to some factor outside the economic system itself," or else "explain them in a way which denies in its root some basic assumption of theoretical economics"; and that "if these explanations of business cycles throw light on the processes of reality, equilibrium economics was barring the way to this light by denying the possibility of general over-production [this point is not discussed further], and adding the dead weight of a barren doctrine to the burdens of a complex reality."

Loewe, we are told, concludes "with a call for building up a body of dynamic economic theory in which business cycles would find their explanation as part of a system" — a challenge which has been accepted by Erich Carrel. Carrel's method of disposing of "equilibrium economics" (his answer to Loewe's "challenge" in the form of his own dynamic substitute is not given) is ingenious, tho it lacks the flavor of novelty. He not only "denies any contradiction between pure economic theory and the generalised reality as it is presented in business-cycle explanations. He goes further and denies the possibility of verification of economic theory by reality. . . . 'Our investigation of the nature of pure (exact) economic theory has shown us that the subject-matter of this science is not an ideal 'system of reality' nor any 'state of economy,' but the essence of 'social economy' considered by itself, the pure 'economy.' We have established that the statements about relations

made by pure theory are not statements concerning causal regularities, but about a causal relation of quantities. The judgments of pure theory relate to the essence or nature of social economy, but not to their existence in reality.'" (Page 391.)

This passage undeniably bears the authentic "metaphysical" label. But it seems possible to make a good guess at his meaning. "Pure theory" is a system of necessary truths — given the premises, the conclusions are the logical outcome thereof. But what are we to make of the suggestion that "the essence or nature of social economy" has no "existence in reality"? Dr. Kuznets himself is apparently somewhat uneasy on this score; for, after endorsing Carrel's views, he adds: "Pure theory is realistic in the sense that it deals with scarcity and rational economic behavior, both largely real entities, i.e. entities found in actual life. But beyond that, all the established relations are in the nature of logical necessities derived from these two assumptions, and not generalisations either of the short, long, or any run of economic processes in real life."

If "scarcity and rational economic behavior" are both (!) "largely real entities," if these are the assumptions from which "pure theory" starts, and if the conclusions of "pure theory" are a body of necessary truths, following logically from the premises, it is a little difficult to perceive why these conclusions should be "without relevance to reality." And it is not so far apparent that History is dealing hardly with "the current generation of economists" in allotting them the task of "starting with the concepts they were taught, and introducing the necessary complications to secure a better fit to reality."

As a matter of fact, however, it would seem that, at least on this level of the problem, it was to the *last*

generation of economists that History delegated this duty. The whole of the monumental life-work of Alfred Marshall is abundant evidence of this fact. We inquired earlier why, if it is possible for "pure theory" to select as premises (say) scarcity, economic "rationality," and the satiability of particular wants, it is not possible for a somewhat less "pure" theory to go "beyond that" and introduce additional assumptions which are likewise "largely real entities." As Dr. Kuznets himself admitted, Lederer was unable to tell us: it does not appear that either Loewe or Carrel has been any more successful.

III

It may be doubted whether, in a discussion which purports to consider the methodological desirability of the course imposed upon present-day economists by the temporal and educational limitations to which they are subject, the "seductive charm" of the economic doctrines in fashion in the first half of the nineteenth century justifies their being placed in the forefront of the argument. Dr. Kuznets himself seems to feel this. "One may doubt," he thinks, "that the economic theorists of today will subscribe to this characterisation of their general systems of economics. . . . The theoretical system of Marshallian economics is certainly not supposed to be a metaphysical discussion of the essence of economy. Neither does Cassel's theory of prices purport to be this." "Of course," he adds, "one might say that none of these systems is an example of pure economic theory, but that is quibbling about definitions."

Why so? If, to qualify as an example of "pure economic theory," a body of doctrine must select only such

elementary "real entities" as scarcity, economic rationality, and the satiability of particular wants; and if the systems of Marshall and Cassel find perhaps their main characterisation in the very fact that they have introduced ever so much more in the way of "real entities" than these things; it would seem that the suggestion that "none of these systems is an example of pure economic theory" is a highly pertinent one. But to take this suggestion up and examine it, to explore its significance for the methodological problem that is supposed to be under discussion, would necessitate an abandonment of the pastime of shooting darts at the seductive charms of early nineteenth century economics.

Such a course, however, besides possibly throwing some light on our present-day difficulties, would have the additional advantage of enabling Dr. Kuznets to escape the thraldom of that "circular reasoning" in the toils of which we have already suspected him to be struggling. For it turns out that the reason why he is content to pass the suggestion over as a mere verbal quibble is that the systems in question "continue to use the concept of equilibrium as a basic element"!

He concedes, indeed, that a certain limited use of the concept of equilibrium "is in itself not objectionable." It may be employed quite innocuously, he thinks, as a kind of all-embracing truism, to depict an instantaneous cross-section of the economic system. Unfortunately, "current economic theories" do not confine themselves to so harmless a use of this dangerous concept. "For, were they to do so, these systems would amount to nothing but lists of factors arranged on the side of supply or on the side of demand, lists of factors whose *interrelations* change with every single change of every single price on the market. They would amount, then,

to what static economics should essentially amount to — a decomposition of social phenomena into the individual acts that underlie them. But they would have no relevance to any *persistent* relations between factors."

It is difficult to understand how the various elements in each successive instantaneous cross-section of the economy can be functionally related, if these functional relationships "have no relevance to any persistent relations between factors" — unless, indeed, Dr. Kuznets intends to suggest that the functions are mathematical only, and not causal; so that the true causal relationships through time (of which the cross-section functional relationships are mere "epiphenomena") have to be sought elsewhere. Such a view, if one finds it intelligible, may be thought successfully to emasculate the equilibrium concept. What it does do, of course, is to present an emasculated version of that concept — leaving it an entirely open question whether some more virile form of it is not also tenable, and necessary to an explanation of dynamic change. This, one may be pardoned for repeating, is the real question at issue: yet nowhere in his article does Dr. Kuznets attack it any more directly, or by any other methods than we have so far found him to employ.

Dr. Kuznets concludes that, "whatever the interpretation of the equilibrium approach, it seems to be a blind alley from the point of view of business cycle theory." But this (as we have seen, essentially unsupported) dictum will scarcely satisfy the modern "theoretical economist." Modern economic theory employs the concept of general equilibrium (or of "the stationary state") as *a conceptual instrument* to give clarity and precision to our understanding of *forces actually opera-*

tive in the real world. It does not claim that these forces ever have attained — or ever will attain — that particular balance which is called static general equilibrium. It does claim that the formulation of the conditions under which these forces *would* achieve such a static balance is a useful aid in endeavoring to understand why, in other and actual combinations, these real forces produce the combined dynamic effects which they do produce: as Marshall put it,[7] "Statics is really but a branch of Dynamics." It was this claim that Dr. Kuznets supposedly set out to dispute. It would have been natural to expect that his course under such circumstances would have been to subject the life-work of the leading economists of the last generation to a rigorous scrutiny, with a view to proving that they were definitely in error in supposing that the equilibrium concept could be of any aid to them in grappling with the complexities of dynamic economic life. Instead, he has confined himself to ringing the changes on the obvious tautology that in a system from which all possible causes of movement have been excluded no movement can occur. His whole discussion up to this point, therefore, is simply one long, elaborate *petitio principii.*

As a matter of fact, it would not be possible to discard the concept of static equilibrium without at the same time throwing aside the whole problem of "economic dynamics" as well. The term "dynamics," like the term "statics," is in economics a "mechanical analogy," and both alike apply to those economic *adjustments* to which the mechanical analogy is applicable. The very term "adjustment" (the use of which and its equivalents Dr. Kuznets appears unable to avoid) implies the concept of equilibrium.

7. Principles, p. 366, n.

But "the Mecca of the economist lies in economic biology rather than in economic dynamics." [8] Dr. Kuznets, tho in one connection, as we have noted, referring to "technical progress" as something "outside" the economic system, in another [9] refers to it as "a dynamic principle." Unless this term be taken to mean "a non-mechanical active principle leading to dynamic reactions," he is, in the opinion of the present writer, wrong in both instances. Technical progress *in itself* belongs to the sphere of economic biology — or to what I should prefer to call Evolutionary Economics. That is to say, the phenomenon of technical progress, like many other phenomena frequently characterised as "dynamic," belongs to the sphere of Historical Sociology on its economic side, and is not itself explicable as the resultant of "mechanical" forces. What *is* properly "dynamic" are *the quantitative adjustments in the system that supervene in consequence of the appearance of some new fact of technical progress (or other form of social change) and of the mutual interdependence of the various elements in the system.* Indeed, all changes in demand (i.e. in the position or shape of the demand curve) are likewise *in themselves* outside the dynamic category. They are super-mechanical social facts, and never fully explicable in "mechanical" terms. What *is* purely "mechanical" are the adjustments in quantity demanded which result from a change in price when the demand curve itself remains unchanged.

There is danger here, however, of misunderstanding; for the interrelations of Evolutionary Economics with Economic Dynamics are as subtle as those of the latter with Economic Statics. In the various changes in demand and supply that occur in actual life there is a

8. Marshall, Principles, Preface to the Eighth Edition, p. xiv.
9. Page 390, n.

continuous gradation from those which seem an obvious and direct response to the stimulus of adjustments initiated elsewhere in the system, to those which may seem to occur *ab initio*. But in no instance can the "creative" activity of human agents be regarded as occurring totally without relevance to the influence of milieu, and in no instance can the most "mechanical" responses of such agents be explained without reference to the nature of the agent as well as to that of the stimulus to response.

The tendency of modern Mechanics is to eliminate from its purview such "metaphysical" (i.e. "real") entities as "force," etc., and to deal only with the static and dynamic relationships of quantity that result from the nature and interaction of such entities. The narrow confines of Mechanics enable it to dispense altogether with discussion of the reality and basic nature of such entities. But the boundaries of Economic Science have a broader sweep; and the science is complicated by the fact that the entities underlying *its* mathematical theory change their modes of response (in many instances unpredictably) with the passage of time.

It is for these reasons that it is suggested that the boundary between Economic Dynamics and Evolutionary Economics (if it must be drawn) is to be sought through the distinction between elasticity and real changes in supply and demand. Responses due to elasticity are strictly deducible from *assumed* conditions in, or characteristics of, the underlying "real" elements; i.e. these conditions are *already fully given*, and translated into (algebraic or arithmetical) magnitudes; whereas changes in the demand and supply curves involve internal readjustment in the "set-up" or structure of the real elements themselves. But once the quantitative aspect of these changes (observable or

predictable) is also *given*, they also (whether they be intra-organic or environmental changes) can be taken for granted by "Economic Mechanics," and incorporated in the structure of a "mechanical" theory—static and dynamic.[1] The real underlying forces are, however (with the exception of independent environmental changes), equally "organic" whether they operate to restore equilibrium, or to create the conditions of a new equilibrium.

It is, then, precisely the element of "rational" or mathematical necessity, which Lederer and Dr. Kuznets regard as inconsistent with the nature of Economic Dynamics, that justifies, in "statics" and in "dynamics" alike, the so-called "mechanical analogy."

But it is not possible really to separate Economic Mechanics from Evolutionary Economics. While Lederer and Dr. Kuznets have failed to perceive that the complex interrelationships of temporal change, equally with cross-section static interrelationships, being capable of quantitative expression, are therefore also capable of incorporation in an abstract theoretical dynamics, it remains true that, when this omission is repaired, any attempt to make of Economics an abstract ("pure") science would still, if carried to its logical limits, render it indistinguishable from pure mathematics — or at least from those particular formulations of pure mathematics that are applicable to the static and dynamic interrelationships of actual or possible economic phenomena. But these phenomena (the

1. The use of the "demand curve" to express the distinction intended is strictly accurate only on the assumption that all changes in the shape or position of the curve indicate some underlying change in the "nature or intensity of desire"; i.e. it is only strictly accurate for single curves on the assumption of "caeteris paribus." A more generalised statement of the distinction in such terms would require more technical mathematical language.

underlying "real entities") must themselves be studied, not only to determine which mathematical formulations are thus relevant, but also to investigate the social and philosophical significance of these formulations when made. It was thus a sound insight that led Marshall to eschew the (for him) easy attractions of abstract "mathematical economics," and to endeavor, out of a long, wide, and rich observation, to clothe these abstractions with the flesh and blood of real economic and social life.

Economic Dynamics, then, would take the underlying super-mechanical facts of change in the conditions of supply and demand as given, would attempt to measure these changes quantitatively, and then, *in the light of the quantitative nature of the various resistances to adaptation*[2] *inherent in the system*, would attempt to calculate the magnitude and duration of the various permutations through which the system must in consequence necessarily pass. If what is *really* the next step in the development of economic dynamics is to be accomplished — i.e. if these temporal adjustments are to be handled not "qualitatively" (as by Marshall) but quantitatively; and not "piecemeal" (as by Marshall) but as a whole, and so as to show the degree and kind of departure of the system as a whole from equilibrium through successive points in time — then the "equilibrium concept" is likely to assume not less, but more, importance in the economics of the future.

2. I should here have employed the term "frictions" had it not been for the recent privilege of reading in manuscript an article entitled "Statics and Dynamics," by Professor F. H. Knight, in which the analogical inappropriateness of this term, as frequently employed in economics, is pointed out, and attention called to the need for distinguishing, on "the mechanical analogy," the different types of "friction" from one another, and friction in general from "inertia." I have therefore employed the term "resistances to adaptation" as a blanket term to cover all causes for the fact that economic adjustments are not instantaneous but occupy time.

But the difficulties in the way of so ambitious a task have long appeared insuperable. Marshall, writing as long ago as 1898, illustrated them as follows:[3]

> ... when a force moves a thing on which it acts, it thereby changes the force which that thing afterwards exercises. The attraction of the Earth alters the movements of Venus, and thus alters the force which Venus exerts on the earth: which again alters the movement of the Earth, and therefore the attraction which the Earth exerts on Venus; and so on in endless but ever-diminishing reciprocal influences. Meanwhile both planets disturb slightly the Sun, whose attraction is their chief controller; and all the other planets have a part in the play. For such complications as these arithmetic is useless: they need the strength and delicacy of vast and subtle mathematical engines working out large volumes full of mathematical formulae and figures. But these engines cannot be applied to economics. . . . Thus, then, dynamical solutions, in the physical sense, of economic problems are unattainable.

It would appear that the existence of a problem of "economic dynamics" was not entirely unknown even to "theoretical economists" of the last generation who "operated with" the "concept of equilibrium as a basic element."

IV

Before offering any personal suggestions as to whether "dynamical solutions of economic problems" are in *any* sense attainable (along the two lines suggested above) to a higher degree than that already reached, we may return to the closing portions of Dr. Kuznets' article. For having concluded, as we have seen, that "the equilibrium approach . . . seems to be a blind alley from the point of view of business-cycle theory," he proceeds to offer a brief discussion which, he claims, in outline at least, "synthesises the recent developments in general economic theory and business-cycle theory,

3. Economic Journal, March, 1898, Distribution and Exchange. Reprinted in Memorials of Alfred Marshall, under the title, "Mechanical and Biological Analogies in Economics."

and, what is more important, calls upon economic theorists to follow a much more promising direction than was indicated by equilibrium economics and its equational treatment of the system."

Dr. Kuznets begins by outlining a recent discussion by Rosenstein-Rodan. According to Dr. Kuznets, this writer holds that "the general theory of equilibrium . . . assumes that there exists an equilibrium price for every commodity, and a certain general equilibrium price-level for the whole economic system. The nature of changes in supply and demand is then such that every disturbance provokes a reaction which compensates it and restores the equilibrium."

To begin with, this is a thoroughly misleading way of characterising modern general economic theory: the "modern" doctrine is that every disturbance provokes a reaction which *tends* towards a new equilibrium. Or, as Marshall has it:

> The theory of stable equilibrium of normal demand and supply helps indeed to give definiteness to our ideas; and in its elementary stages it does not diverge from the actual facts of life, so far as to prevent its giving a fairly trustworthy picture of the chief methods of action of the strongest and most persistent group of economic forces. But when pushed to its more remote and intricate logical consequences, it slips away from the conditions of real life. . . . For though the statical treatment alone can give us definiteness and precision of thought, and is therefore a necessary introduction to a more philosophical treatment of society as an organism; it is yet only an introduction.[4]

Rosenstein-Rodan does not definitely suggest that "general economic theory" entertains the possibility that all reactions are instantaneous — tho he does, it seems, "go on to show how the time element came to be omitted in equilibrium economics." [5] It was

4. Principles, p. 461.

5. "The element of Time . . . is the centre of the chief difficulty of almost every economic problem." Marshall, Principles, Preface to the First Edition, p. vii.

"omitted" in the sense that "it was implicitly assumed . . . that the whole equilibrium system and its implications rests upon the assumption of an equal rhythm of changes." We have already sufficiently indicated that such an assumption is in direct contradiction to to the whole body of Marshall's teaching.

Rosenstein-Rodan points out three "possibilities": "1. The time coefficients are all equal: the equilibrium is attained directly. 2. They are not equal, but become equal after several transformation periods: equilibrium is attained only after a certain period. 3. They are not immediately equal and never become so: there is never an equilibrium."

The first of these, as Dr. Kuznets has no difficulty in showing, is hopelessly out of accord with reality. The second he disposes of as follows: "But even this case is not in agreement with reality, mainly because of the cumulation of random changes which takes place whenever there is an original inequality of time coefficients."

I am not quite sure what is meant here. If, however, it refers to the fact that, during the period of adjustment, *new* changes of what I have called the "evolutionary" type occur, thus superimposing a new set of dynamic reactions upon those already under way, Dr. Kuznets is coming dangerously close to adopting the accepted Marshallian doctrine regarding the nature of dynamic change and the function of the "equilibrium concept."

Even then, however, the qualification "mainly" weakens suspiciously this accurate statement of the case. It may be well to bring out one point, lest Rosenstein-Rodan and Dr. Kuznets should be laboring under a misapprehension. If the conditions of a stationary state were actually given, without, however, general equilibrium having been yet attained, and if no new evolutionary stimuli to further dynamic reactions

supervened during the period of adjustment, the original "inequality of time coefficients" would not of itself prevent progressive adjustment towards a position of final equilibrium. This point was not overlooked, strange as it may seem, even by Cournot:

> When an economic system is in equilibrium any perturbation, according to Cournot, will set up primary or direct effects, which are limited to the immediate object disturbed, and secondary or indirect effects, which result from the *liaisons* between all the elements in the system. In consequence of these functional *liaisons*, the indirect effects are diffused throughout the entire system in the form of oscillations which "*the general principles of analysis will show us must go on with gradually decreasing amplitude.*" [6]

Marshall, in his astronomical analogy already quoted, expresses the same idea with the phrase, "and so on in endless but ever-diminishing reciprocal influences."

The true basis of distinction, therefore, between Rosenstein-Rodan's second and third "possibilities" would seem to be the continued presence, or the absence, of the underlying conditions of the stationary state. Since the assumption of the existence of these conditions is simply a convenient scientific fiction sometimes employed by "theoretical economists" for purposes of analysis, Dr. Kuznets, in concluding that it is the third of Rosenstein-Rodan's "possibilities" which corresponds to reality, is allying himself with the best traditions of modern general economic theory.

It may be worth suggesting, however, that a sudden vivid perception of the intricacies in the interactions of adjustments characterised by "inequality of time coefficients" may, unless one is on one's guard, lead to a species of intellectual hysteria, in which "the theory of stable equilibrium of normal demand and supply,"

6. H. L. Moore, Synthetic Economics, p. 27. Cournot, Researches into the Mathematical Principles of the Theory of Wealth, Bacon's translation, pp. 130–131.

so far from "giving a fairly trustworthy picture of the chief methods of action of the strongest and most persistent group of economic forces," is felt to have no bearing whatever upon "generalisations either of the short, long or any run of economic processes in real life." The resultant vision of a chaotic, lunatic world can be exorcised either by sober reflection or (the "theoretical economist" may suspect) by the methods of "empirico-realism." It may be suggested, for example, that if Dr. Kuznets were to select — quite at random — any two economic products (say, an automobile and a collar-stud), and were to compare over a number of years (without eliminating either secular trend or cyclical fluctuations) their respective costs of production with their respective market prices, he would obtain a much higher correlation, in the approximation of price to cost, than if he were to compare the cost of production of the one with the market price of the other.[7]

In his next section, indeed, as the "economic theorist" will note with satisfaction, Dr. Kuznets proceeds to make use of the equilibrium concept in outlining his theory of business cycles. But we shall have to notice that even then he makes one last (perhaps in this case unconscious) effort to dispense with this concept in the very process of professing to utilise it.

V

The business cycle theory which Dr. Kuznets develops in brief outline is, if I have not misunderstood it, essentially as follows.

7. One might refer also to the authoritative pronouncement of Dr. Mordecai Ezekiel, in his contribution to the discussion of Professor F. C. Mills' paper on Economic Dynamics at the 1929 Meeting of the American Economic Association. *Vide* Supplement, American Economic Review, March, 1930.

In the first place, the fact that the time-spans of the various readjustments towards equilibrium are unequal, combined with the fact that, as Dr. Kuznets phrases it, "a continuous stream of random events impinges upon the economic system," makes it *possible* for that system, through "cumulation" of the dynamic reactions to these events, (*a*) to depart very considerably from equilibrium, and, (*b*) in consequence, to require some considerable period of time to readjust itself again.

With this no "economic theorist" is likely to quarrel: so far Dr. Kuznets is certainly remaining true to the implications of his unconscious Marshallian background.

In the second place, such prolonged fluctuations away from equilibrium are not merely possible: a continual succession of them is an actual historical fact. The problem of "the business cycle" is the problem of explaining why and how the intricate interactions of temporally unequal adjustments towards equilibrium in fact produce this succession of "cyclical fluctuations."

There are, says Dr. Kuznets, two reasons for this. First, the multiplicity of incitements to adjustment are not all of very small individual significance, but have, scattered through them as it were, a smaller number of relatively important ones — "important," that is, in the sense of involving much more serious departures of the whole system from equilibrium, and (consequently) much more extensive readjustments towards equilibrium, than do, individually, the larger number of minor incitements. (Concrete examples of such exceptionally important disturbing factors would presumably be — a basic constructional invention, or a war.)

Secondly, in the absence of such abnormal single incitements to the occurrence of "cycles," we may

expect the occasional appearance of "clusters" of individually less important disturbances whose combined effect happens to be, through the fact that they are somehow "cumulative," the creation of extensive economic dislocation. (Concrete illustration here would require, not merely a listing of several contemporaneously possible minor incitements to readjustment, but an indication of why this particular group, occurring close together in Time, should have 'cumulative' effects. Dr. Kuznets may know if the following would be a valid illustration: the Hatry failure, an unprecedented spread of the desire to speculate on the New York Stock Exchange, abuses of the practice of instalment selling, over-production of automobiles, and a psychological condition of painful uncertainty regarding the nature and direction of imminent alterations in the United States tariff. I suspect, however, that for purposes of scientific discussion he would wish to break these items up into smaller units — a consideration which raises the question of what we are to consider as an "incitement unit.")

If, now, we ask why such (single or complex) incitements to cyclical fluctuation should occur with such approach to regularity as is required by the actual business cycles of history, the answer is that it is of the nature of "normal" or "random" frequency that this should be so. The Russian statistician, Eugen Slutsky, has, we are told, recently shown that the moving average of "a normal frequency distribution strung along the line of time will not result in a straight smooth line, but in oscillations." This is because of the more or less regular recurrence (*a*) of individual items with an abnormally large deviation from the mean, or (*b*) of large clusters of items which, though individually closer to the mean, happen all to have "the same sign." Since,

therefore, (*a*) we have no reason to expect *all* incitements to economic adjustment to be of equally small "importance" as disturbers of equilibrium; and since (*b*) we have likewise no reason to expect the more numerous, individually less "important" ones, occurring at any one time, invariably to be of such a combined character as *not* to involve serious economic dislocation; it follows (*c*) that we *have* reason to expect, on general probability grounds, that (single or complex) incitements to unusually extensive departures from equilibrium will occur at intervals of an unspecified degree of regularity. We may say (if we wish to speak pictorially) that adequate initiating causes of "business cycles" will themselves tend to occur in "cycles."

With this thesis also (subject to the qualification regarding the difficulty of defining an "incitement unit") I can see no reason for any "economic theorist" to quarrel. It has the merit, too, as Dr. Kuznets himself points out, of expressing in the sharp language of Probability theory the fact that "we do not need the hypothesis" — of the regular recurrence of an identical exciting stimulus — "which is deemed necessary by some theorists of business cycles." [8]

Repeated perusal of this section of Dr. Kuznets' article has, however, convinced me that he is there offering something more than this, after all, somewhat simple thesis. He appears to assume without question that, from this demonstration of the statistical probability

8. Apparently the reference here is to such theories as "the sun-spot" theory of Jevons. It is perhaps unnecessary to point out that while the above thesis shows clearly that the hypothesis of such recurrence of an identical stimulus is not essential to account for the fact of business cycles, there is nothing in it to show that such recurring stimuli are absent. From the "empirico-realistic" standpoint, therefore, the question whether there may not be some truth in such theories remains an open one.

of the "cyclical" occurrence of initiating stimuli adequate to produce economic "cycles," it somehow follows *that the mechanism of the economic "cycles" themselves is identical with the "mechanism" of a moving average.*

It is to be seen that the so-called institutional explanations of business cycles deal mainly with the economic forces that make for cumulation, with forces that explain why a given random event is not immediately cancelled by an opposite reaction but allowed to exert its influence for some time to come, an economic counterpart of the statistical mechanism of moving average. (Page 410.)

He adds, indeed, that "the preceding discussion (of "differences in time coefficients") makes the importance of this statistical thesis still greater. But this is to misstate the relation of "the statistical thesis" to the "inequality of time coefficients." The "remarkable thesis" of Slutsky that "cumulation of random changes becomes in certain conditions a source of prolonged oscillations" simply means that in a normal frequency distribution exceptional deviations (single or complex) will recur at intervals, and that if a moving average of such a frequency distribution be plotted diagrammatically it will appear as a wave-like curve, or series of "cycles," owing to the process of averaging. But, so far as the "business cycle" is concerned, this has reference only to the probable "cyclical" occurrence of *stimuli adequate to produce "cycles."* What makes these stimuli "adequate to produce (economic) cycles" is the fact that they (*ex definitionis*) somehow or other, in consequence of the temporal inequality of adjustments toward equilibrium and of the continual occurrence of new stimuli, lead to "cumulative" departure of the economic system from general equilibrium; and therefore also — because the forces making for equilibrium operate but slowly and imperfectly — to an only gradual return towards equilibrium. But for a *theory* of the

business cycle it is not enough to demonstrate the statistical probability that stimuli "adequate to produce it" will occur with the requisite degree of regularity: we must also analyse the nature and operation of the "cumulative" reactions of the economic system to these stimuli when they do occur (which would seem to be what the "so-called institutional explanations" are trying to do). Between such economic "cumulation" and the "cumulation" of the moving average the only obvious similarity would seem to be that, when diagrammatically represented, they both "go up and down."

It would seem to be essential sharply to differentiate three distinct things: (*a*) fluctuations in the degree of seriousness of successive (single or complex) incitements to economic disequilibrium; (*b*) fluctuations of economic time-series above and below their respective trend-lines; and (*c*) "fluctuations" of the economic system away from a position of *real* general equilibrium.

If the vast multiplicity of individual incitements to economic adjustment were each assigned numerical values indicative of their individual potentialities to produce disequilibrium, these numerical values might, we agree, take the form of a normal frequency distribution "strung along the line of time"; so that, if a moving average of these values were computed, and depicted diagrammatically, it would display "cyclical oscillations." But these "cycles" would not *be* economic cycles: they would merely be, at best, "cycles" in the occurrence of stimuli adequate to produce economic cycles. And in fact they would not even be that — unless it were possible, in some manner not apparent to me, to make these numerical values indicate, not merely the intrinsic "disturbing power" of each incitement considered in itself (and it is more than

doubtful whether there is any meaning in this), but also when a number of them in combination were adequate to produce an economic cycle. The trend-line of this frequency distribution would otherwise indicate merely the average individual disturbing power of the incitements considered discretely. The occurrence of "clusters" of "the same sign" would indicate that all the items in a cluster (considered separately) had more (or less) than the average disturbing power of all individual items. It would tell us nothing of whether they nevertheless tended to "offset" one another as incitements to economic cycles.

If the apparently inherent difficulty of this discrete statistical treatment of individual incentives could be somehow overcome, so that the "cyclical oscillations" of our moving average really did indicate fluctuations in the degree of seriousness of successive *combined* incitements to economic disequilibrium, an upward or downward inclination of its trend-line would then indicate that the economic system was becoming progressively more or less unstable. The "peaks" in the fluctuations of the moving average would indicate the presence of *maximum* combined incentives to disturbance — i.e. presumably, of incentives "adequate to produce economic cycles." The "troughs" in these fluctuations would indicate the presence of *minimum* combined incentives to dislocation — i.e., presumably, the expectation of a *maximum* approach to real equilibrium on the part of the economic system itself.

The trend-lines of (*b*) indicate something quite different: secular growth or decline in the economic magnitudes whose cyclical fluctuations are depicted: volume of production, volume of employment, volume of bank clearings, rates of interest, and so on. These time-series may, or they may not, be presented as moving

averages. Even when they are not, they still exhibit cyclical up-and-down swings.

Moreover, these cyclical fluctuations in time-series are merely the empirical results or outward manifestations of the economic "cycle" — "raw materials" which a business cycle theory would presumably attempt to explain. The "equilibrium economist" believes that this explanation is to be sought through the hypothesis (founded on over a century of wide and varied empirical observation) that the economic system, during successive points in Time, departs in varying degrees — for reasons whose general nature Dr. Kuznets, following Rosenstein-Rodan, has himself indicated — from a state of *real* general equilibrium; towards which, however, *real* forces are continually and persistently "tending" to bring it back, with varying degrees of success. It seems fairly clear from our preceding discussion that Dr. Kuznets (whose attacks upon the "equilibrium economist" turned out to arise merely from anxiety to show that we do not, in fact, inhabit either a static, or a continuously equilibrated dynamic, world) is really of the same opinion. One is entitled to expect, therefore, that his business cycle theory — especially if, as promised, it is to provide a "synthesis" with "recent developments in economic theory" — would make specific use of these real forces. If, on the other hand, we are to take seriously his other statement that "the equilibrium approach . . . seems to be a blind alley from the point of view of business cycle theory," we should expect him to affirm boldly that any "synthesis" of economic theory with business cycle theory is consequently impossible; to show why it is impossible; and to state, and clearly differentiate, the alternative methodology that he proposes to employ. But he has done neither of

these things, and one suspects that this is because he has been misled, by the fact that the moving average of a normal frequency distribution (when diagrammatically represented) moves "up and down" like the time-series which are the raw materials for a theory of business cycles, into supposing that "the equilibrium approach" is thus somehow automatically "synthesised" with some other and perhaps quite inconsistent approach which he does not define.

A general index of the quantitative departures (at successive points in Time) of the economic system from general equilibrium would be, if anything, even more difficult to conceive and construct than a general "business cycle index" in the ordinary statistical sense.[9] *There is no reason to suppose that it would coincide with the latter.*

The constituent time-series of which an ordinary statistical general business cycle index would be composed have, as Dr. Kuznets points out, upward or downward secular trends. The precise form of these trends depends in part on how they are computed, for they are derived from the "fluctuations" themselves. A statistical synthesis of these time-series would again be in large part arbitrary,[1] but we may assume that it would exhibit a general "upward" tendency for an expanding economy. An index of quantitative departures of the economic system from general equilibrium would also (assuming it could be constructed) have a "trend-line." But this trend-line would be indicative of the average degree of departure of the economic system from general equilibrium: unless the system

9. On the difficulties and limitations of the latter, see Wesley C. Mitchell, Business Cycles: The Problem and Its Setting, pp. 320–324.

1. It could conceivably be logically constructed on one consistent basis, but the selection of the basis would, from the "equilibrium" standpoint, be arbitrary. Cf. Mitchell, loc. cit., p. 322.

were becoming progressively more or less unstable, this trend-line would be horizontal. Furthermore, comparisons would be made, presumably, not with the trend-line at all, but with a straight line, necessarily horizontal, indicative of sustained equilibrium. There would be no meaning in the expressions "above" or "below" equilibrium; only in the phrase "away from" equilibrium; and the "fluctuations" would have to be exhibited entirely on one side of the equilibrium line.

We have no ground for expecting that, if a "moving" general equilibrium could be somehow achieved, the behavior of the various statistical time-series would correspond to that of the lines of trend, however constructed, of the same series in an unequilibrated world.

A real "static" general equilibrium (as distinct from Dr. Kuznets' merely nominal or equational one already discussed) would be a static balance of real forces consequent upon the Principle of Substitution having completely worked itself out in a world in which the underlying conditions of the stationary state were permanently given. A real "moving" general equilibrium (if it could occur) would be one in which *the manifold progressive dynamic adaptations of a changing world synchronized perfectly with the evolutionary changes that provoked them;* in such manner that, at each successive point in Time, the system was in general equilibrium relatively to the underlying conditions as given for that instant. Such realistic considerations as the continual change in relative costs, and in relative demand, suggest that the "fluctuations" of some time-series in a real moving general equilibrium might well be complex and varied.

This conception of a real moving general equilibrium is not synonymous with the assumption that all economic adjustments occur "immediately"; nor yet with

Rosenstein-Rodan's "first possibility" that "the time coefficients are all equal" (a "possibility" which would appear in many instances to necessitate the part being equal to the whole). It involves rather a complicated and miraculous dovetailing of the various temporally unequal adjustments with one another, and with the various evolutionary changes to which they are the responses. A less strict, more "pragmatic," interpretation would doubtless permit *some* "lags" in adjustment: i.e. temporarily "sub-optimum" employment of resources. But it would presumably be compelled to rule out definite *unemployment* of resources of any kind, even for short periods; otherwise the conception of a moving general equilibrium would cease to have meaning, and, if nominally retained, become simply a stringing together on the thread of Time of successive static "equilibria" of the meaningless, tautological type earlier visualised by Dr. Kuznets. But it is clearly not this sort of "equilibrium" that Dr. Kuznets himself has in mind in the present connection when he speaks of "changes around the equilibrium" — for from such a "moving equilibrium" there would be no such things as "departures."

The possibility of actually achieving a real moving general equilibrium may well seem remote. It is worth while to point out that it might not be desirable. For even if the continual "maladjustment" of the actual world could somehow be eliminated, the optimism of Bastiat would still not be justified: each successive equilibrium would be conditioned by its predecessors, and all alike by the desires, decisions, and actions of human beings. If these were shortsighted and foolish, the moving equilibrium would still fall short of the economic ideal; for the community would be imposing upon itself a series of equilibria which, though "op-

tima" for the brief moments of their successive existences, yet, estimated as a continuous whole over a period of time, might well be less "desirable" than a course of events, during the same period, involving departures from equilibrium. A real moving general equilibrium (or even the closest possible approximation thereto) is not, therefore, a self-evident and sufficient goal for social policy. It represents, however, as being the continuous optimum employment of resources, the theoretical standard, and usual practical goal, implicit behind the desire to "control the business cycle." And it may perhaps be safely assumed that, *caeteris paribus*, any reduction in "cyclical" departures from such a real moving general equilibrium is "economically desirable."

All these various strands in the problem obviously require careful unravelling before intelligent discusson is even possible — let alone before we can hope for any "synthesis" of economic theory with the empirical study of time-series. It is not necessary to show in detail their inextricable intermingling in Dr. Kuznets' discussion. Two examples may serve, however, to indicate the general nature of the confusion.

The statement (page 409), "Since we conceive the random events as changes around the equilibrium, their average is zero," is really devoid of meaning. The events themselves (wars, technical developments, crop changes, my sudden decision to give up smoking) cannot be conceived as changes round an equilibrium, nor averaged out to zero. If they were broken up into their constituent "incitement units" (changes in demand and supply schedules for particular commodities and services?) and their individual "disturbing power" (itself a conception of doubtful validity) given quantitative expression, these quantities would average

out to zero only if expressed as deviations from their own trend-line, which denotes, not equilibrium, but the average individual disturbing power of the "incitement units."

Take again Dr. Kuznets' "first complication" (page 411).

> There is a certain direction in the random changes, i.e., a trend movement. For example, such outside events as technical changes which for the present purpose we may conceive of as random, have a certain trend. Their economic significance or influence tends persistently in one direction, that of lowering costs. Changes in population also are not purely random. . . . They tend . . . in the direction of positive growth. We may conceive the random changes as belonging to a distribution skewed in one direction, a distribution which, strung out along the line of time, presents deviations not from a straight horizontal line, but from a line which runs either upwards or downwards.
>
> This skewness . . . is responsible for the presence of secular movements.

Not only is the trend-line of the random changes here again confused with the "equilibrium line," but this in turn is confused with the secular trends of statistical time-series.

We conclude that the "mechanism" of the moving average is possibly adequate (if the analogy is not pressed too hard) to the task of showing that, in a world in which we have no reason to expect all "random changes" to have equally negligible disturbing power, and no reason to expect these disturbances invariably to "offset" one another immediately, there is a probability that changes with an unusual power of disturbance, or, alternatively, "clusters" of changes with "cumulative" powers of disturbance, will crop up at intervals; but that this "mechanism" is only a spurious and misleadng analogy to the actual mechanism through which these stimuli operate when they do occur.

Of the fact that the economic mechanism cannot

really be explained in terms of random frequency at all, Dr. Kuznets appears to have some inkling in his "second complication" when he says: "The second complication is that we cannot treat the stream of random changes as continuously random, for the simple reason that, after the cumulation of these changes has gone on for some time, and has yielded an upward or downward movement of some duration, the existence of this movement becomes in itself an important factor, and begins to exercise an influence which overshadows the continued play of further random changes." (Page 412.)

If he had gone further and pointed out (*a*) that the various "random changes" are only very imperfectly "random" from the outset, since they, to a large extent at least, promote one another, and (*b*) that the *return* to (or towards) equilibrium is not the outcome of the eventual "random" occurrence of events with "an opposite sign," but of adjustments which are inevitably *forced* by the operation of "the strongest and most persistent group of economic forces," he might have been led to doubt very seriously either the explanatory or the clarifying power of his statistical analogy. Indeed, if a troublesome over-subtlety seems still to cling about the distinction between stimuli adequate to produce cycles and the cycles themselves, the key to this difficulty lies in the inappropriateness of the term "random changes" itself, and, consequently, of this whole mode of approach to the problem.

This difficulty may be further characterised by saying that it arises from his misuse of the distinction between the "system" and "outside" changes. For, curiously enough, Dr. Kuznets has himself turned out to be a disguised adherent of the "independent variable" type of business cycle theory. On his view (tho not on mine),

"to interpret business cycles in this way is to confess the failure of economic science to explain their appearance." Hence, no doubt, the resort to Probability and the "mechanism" of the moving average.

If (*a*) for "random events" we substitute the term "evolutionary events"; if (*b*) we recognise clearly that — while these may be "random" in the very limited sense that they are not the combined, controlled result of the action of some central omnipotent intelligence aiming at a "moving" general economic equilibrium — they are nevertheless mutually interrelated both "cross-sectionally" and through Time; and if (*c*) we do not overlook the *probability* that these concatenations of "stimuli" to economic change, despite the fact that they are never identically repeated, yet arise in successive business cycles in certain broadly definable general forms and operate through certain fairly well-defined channels — we shall be led to suspect that the "so-called institutional explanations" are more likely to achieve soundness both in methodology and in results if they hold aloof altogether from any entangling alliance of the kind suggested with the "mechanism" of the moving average.

VI

It should be clear without any further elaboration that we have been disappointed of our "synthesis" of "recent developments in general economic theory and business-cycle theory." The "recent developments" in general economic theory, if by these are meant the "establishment" of "the inequalities of time-coefficients," are not recent: they go back at least as far as Cournot; and, in fact, it is highly questionable whether anyone ever doubted them. The "recent develop-

ments" in business cycle theory, in so far as by these is implied the analogy with the "mechanism" of the moving average, are not acceptable; nor is this analogy consistent with (or able to be synthesised with) the conception of a continual attempted adaptation towards real equilibrium which is continually thwarted in some degree by the inequality of time-coefficients *plus* the constant emergence of new evolutionary changes. It is on the basis of this latter conception alone that any such synthesis is possible.

In Dr. Kuznets' last section, soundness of instinct triumphs over consistency, and he returns (page 414) to a direct consideration of temporal inequalities in adjustments towards equilibrium. He proposes, if I understand him rightly, (*a*) the classification of individual responses into economic or "social" groups — on some basis not revealed; (*b*) the study of the mass responses of these various groups with a view to ascertaining (*i*) the order of magnitude of their respective time-coefficients, (*ii*) the reasons therefor; and (*c*) a study of the behavior of the secular trends of statistical time-series.

So far as it goes, this program is admirable. But it would not be difficult to show that the main preoccupation of practically every reputable writer on business cycles during the last twenty years has been with the temporal inequalities (and disproportionalities) of types of economic response; with the reasons therefor; and with their tendency, under certain conditions, to produce "cumulative" departure from equilibrium. Probably no one doubts that a more intensive, systematic empirical study of these things would be valuable; and there is a very real advantage in thus formulating in general terms the nature of the basic elements in any business cycle theory — or any

economic dynamics. But unless their more intensive investigation is to occasion much waste of time, effort, and money, it must be undertaken with a little more theoretical leavening than is indicated by Dr. Kuznets.

And it is certainly not clear in what way this program "calls upon economic theorists to follow a much more promising direction than was indicated by equilibrium economics and its equational treatment of the system." On the contrary, it is difficult to see how such an empirical program, inspired as it really is by "equilibrium economics," can hope to achieve very much without continued further close coöperation from that despised quarter. Dr. Kuznets' own criticism of the "equational method" is in fact applicable with much greater force to a one-sided empiricism:

"It may be suggested that the whole intellectual procedure is well adapted to a science of controlled experiment, where isolation of factors is possible (to a considerable degree) and the instantaneous performance provides an immediate check, but is dangerous in a science whose subject matter is a changing flux of complex phenomena, bound historically and exhibiting only the loosest form of regular sequences." (Page 400.)

If the attempt to find a new equilibrium is really the common motive power behind the multitudinous variety of the dynamic reactions of actual economic life,[2] it is on the basis of this notion that we must hope to build a more adequate theoretical dynamics; and it is on the basis of this notion that any future policy of social control must be constructed. The dynamics which we already possess is thus founded, and nearly all our present rudimentary attempts at social control

2. In the case of those human acts which are markedly evolutionary as distinct from "passively" dynamic, it would be more accurate to say that these constitute an attempt to create the conditions for a new equilibrium.

of economic phenomena will be found on inspection to rest implicitly or explicitly on the same notion. Similarly with nearly all aspirations for the future. Behind the idea of "controlling the business cycle" lies, as we have seen, the implicit ideal of the closest possible approximation to a continuous optimum employment of total resources: in other words, the conception of a "moving" real general economic equilibrium. To take this notion seriously would involve something much more fundamental than the separation, in empirical series, of "cyclical oscillations" from "secular trends," and the "behavioristic" study of the various empirical coexistences and sequences of each. It would involve something much more fundamental than the piecemeal investigation of "reasons" for these. It is not questioned that it does involve all these things. But it would involve also nothing short of a scientific "rational" integration of these complex empirical interrelationships with the complex interrelationships that *would* characterise a real moving general equilibrium.

This in its turn would involve (*a*) determination of the conditions under which a moving general equilibrium would be possible; (*b*) determination of the limits of amenability to control of the various types of evolutionary change and the various types of dynamic response; (*c*) determination of the consequent practical limits to any policy aimed at achieving moving general equilibrium; and (*d*) a balancing "at the margin," within these outside limits, of the advantages of each successive step towards moving equilibrium against the possible costs or sacrifices of various kinds entailed by each step.

Moreover, this purely quantitative way of expressing the problem conceals many deeper difficulties. There would also be the problem of weighing against one

another the different kinds (as distinct from degrees) of disequilibrium. And there would be the problem of how far it would be possible and how far desirable so to alter the *structure* of the system that existing economic groups — whose "responses," through limited amenability to control, obstructed social policy — would be modified or replaced by new groupings over whose "responses" better control could be exercised. And behind all these and involved in each would be the question of the real social significance of the economist's conception of optimum employment of resources — a question the satisfactory solution of which would tax the combined resources of the various positive social sciences and of social and political philosophy.

These are some of the problems which a frank facing of the issues raised by the desire to effect and utilise radical improvements in our "economic dynamics" shows to confront "the current generation of economists." If it be really possible to segregate from these those wider issues which indicate the timeliness of present tendencies towards closer coöperation of the social sciences, the remaining "purely economic" aspects of these problems still clearly present a formidable array of difficulties. For these, as for the wider issues, the conception of a moving general equilibrium offers at least a provisional norm in the light of which we may attack the tasks imposed upon us "as a matter of historical development." A radical "empirico-realism," on the contrary, deprives us of all our well-tried bearings, and offers in their place only the vague hope that if we can but amass enough "facts" something may eventually come out of them.

It seems possible that the root of our difficulties lies not in the methodological inadequacy of "equilibrium economics" to cope with the complexities of real life

laid bare by the "business-cycle theorist," but in the human limitations of "economic theorist" and "business-cycle theorist" alike, and their possible inadequacy to cope with problems which, however, must at least be intelligently appreciated before they can be successfully attacked.

VII

There is reason to suspect that not all leading economic statisticians are today satisfied with purely empirical programs for the study of "the various types of economic reaction"; and that some of them feel that even the beginnings of a "rational" body of dynamic theory — in a form more obviously susceptible of employment in conjunction with modern empirical statistical investigations than that of the dynamics we already possess — might be helpful, not only in directing the further course of such investigations, but also in organising, interpreting, and applying their results.[3]

Such questionings deserve serious attention from "the current generation of economists." For while there is, in the perception of the organic unity of "fact" and "theory," nothing essentially novel,[4] we have yet admitted that the remarkable progress in recent years, in the application of statistical methods to the collection and arrangement of economic facts (particularly as regards time-series and their empirical interrelationships), raises the question as to how, if at all, economists of today may achieve an advance on their predecessors in the methods of dynamic analysis. We have suggested, too, that the natural line of further

3. See, e.g., F. C. Mills, loc. cit., passim.

4. "Adam Smith saw clearly that while economic science must be based on a study of facts, the facts are so complex, that they generally can teach nothing directly; they must be interpreted by careful reasoning and analysis." Marshall, Principles, p. 759, n.

progress of the kind under discussion seems to be two-fold: (*a*) to make economic dynamics quantitative instead of qualitative; and (*b*) to make it comprehensive (in the sense of presenting fluctuations of the system as a whole) instead of "piecemeal." But Marshall's vivid appreciation of the difficulties in the way of such a task — as indicated by his astronomical analogy — has also been emphasised. Is his statement, then, that "dynamical solutions, in the physical sense, of economic problems are unattainable," the last word on this subject?

"The most helpful applications of mathematics to economics," he said,[5] "are those which are short and simple, which employ few symbols; and which aim at throwing a bright light on some small part of the great economic movement rather than at representing its endless complexities." It seems possible to accept this statement as it was meant, and still to hold that there is a third alternative use for mathematics as applied to "the great economic movement" — one which does not essay the hopeless task of "representing its endless complexities," and yet performs a function different in kind from that of "throwing a bright light on some small part" of it. The nature and possibilities of this third use have, in the present writer's opinion, been most suggestively indicated in recent years by Mr. D. H. Robertson.[6]

This method consists essentially in selecting certain broad but fundamental categories of economic life (whose "necessary" functional relationships have already been laid bare by the analysis of our predecessors), and developing (in generalised form) the quanti-

5. "Mechanical and Biological Analogies in Economics," loc. cit.

6. D. H. Robertson, Banking Policy and the Price-Level, P. S. King, London, 1926. See also his Money, Second Edition.

tative aspects of these functional relationships through Time, in such a way as to exhibit some of the essential conditions under which alone a moving general equilibrium of the whole system can be maintained; and the degrees and kinds of disequilibrium of the whole that will accompany various departures from these conditions. Such a method (based as it is on the assumption of the essential truth and reality of "equilibrium economics" of "the Marshallian type") will necessarily assume a great deal regarding the "endless complexity" of particular adjustments that underlie the general categories it has selected for treatment. It will also of necessity start from very simple direct assumptions regarding these categories. But, in the first place, such assumptions, direct and indirect, when brought to the light, may be expected to act as starting-points for further more complex and more realistic analysis; and, in the second place, if the consequent progressive selection of categories is made with the requisite practical imaginative insight, apparently very large assumptions of the indirect type (regarding the masses of underlying individual adjustments) may be more permanently justified through the success of the method in laying bare what Professor F. C. Mills has aptly termed "strategic points" in the economic system for the application of social policy. Incidentally, it should help to increase our detailed understanding of the general truth — inadequate regard for which has repeatedly produced unexpected and sometimes painful practical consequences — that the modes of response to treatment of such "strategic points" are not to be understood in isolation, but only in the light of the nature of their mutual interdependence.[7]

7. The suggestion may be hazarded, out of an abysmal ignorance of the subject, that, in view of the recent prominence given to Probability

In this way, what Dr. Kuznets has called "inequalities of time-coefficients," and what I have ventured to term "resistances to adaptation" (towards equilibrium), may be taken *quantitative* account of (they have long, as argued, received *qualitative* recognition), without embarking upon the hopeless task of following them in all their individual ramifications.

Concrete illustration of the nature of this method is possible here only in the barest and simplest form. Mr. Robertson[8] has shown, among other things, that, in a community in which all real saving was financed by the creation of new credit or currency, in which *per capita* productivity was constant, and in which, through the maintenance of a constant average "demand for money to hold," the "period of circulation" of money as income remained constant — given these condi-

and the statistical method by the science of Physics (see, e.g., Eddington, The Nature of the Physical World), "dynamical solutions" of economic problems along the lines here suggested might prove, after all, to be not so far removed from such solutions "in the physical sense" as Marshall in his day and generation may be pardoned for having supposed. But, if this suggestion is valid, the radical development in the matter of Probability has been, not in economic science, but in Physics. The qualitative economic dynamics of the last generation also made use of the Probability concept, being basically concerned, not with individual, but with aggregate, or group, demand and supply schedules. The essence of the economic development here proposed is not the unification, for the first time, of economic theory and economic statistics, but the development of a generalised quantitative (i.e. mathematical) theory of continuous temporal relationships among basic or "strategic" economic categories, as what is really required to make economic theory a more adequate working partner for statistical studies of the time-series type.

What has really transpired, then, if the suggestion hazarded concerning Physics has any merit, is that "dynamical solutions, in the physical sense," have been found, at least for the time being, to be "unattainable" *in Physics*; and that this science now contains, either temporarily or permanently, the analogue of that "uncertainty" resulting from "individual freedom" which economists very early felt themselves called upon to discuss. But it would be a strange confusion of thought to conclude from this that all unravelling of necessary connections among entities has become impossible, simply because these entities are now found to be indefinitely complex where formerly they were thought to be simple.

8. Op. cit.

tions, a uniform expansion of aggregate output (resulting from a uniform rate of growth of the working population) could only take place "on the even keel of a stable price-level" if the ratio of the "period of circulation" to the average "period of production" of all goods were equal to the ratio of the value of the total stock of circulating capital to the value of the total output of final goods marketed during an average production-period. (This implies that if the aggregate value of the circulating capital in existence at the outset exceeds the total stock of money in circulation, some amount of voluntary saving out of current money income, so far from depressing the price-level, is, under such conditions of expansion, necessary to keep it from continuously rising.)

The direct assumptions are here very simple; but it should not be beyond the resources of the mathematical economists to substitute variables for the various constants here assumed, and to ascertain the consequences of such substitutions. And it is here that the possibilities of organic coöperation between empirical statistical investigations and mathematical analysis become manifest. The variations on such a theme abstractly open to the mathematician presumably are practically limitless: adequate empirical knowledge of actual variations (and of the actual order of comparative magnitudes) over long periods may lead to inquiry as to the possible existence in real life of necessary or probable limitations (of perhaps varying degrees of permanence) in these things; and thus may save the mathematical economist from the construction of elaborate "necessities of thought" which *are* "without relevance to reality." [9]

9. In some cases our existing empirical knowledge and past analyses may be sufficient to enable us to deduce such necessary outside limits; in others new empirical investigations or new deductions or both may

Again, the above formula for a simple "moving equilibrium" may be shown to involve the (very large) indirect assumption of the maintenance of "right" proportions in the outputs of individual commodities. But this discovery, so far from stultifying the formula (a consequence that would seem immediately apparent to some minds), suggests the possibility of further elaboration of the theory. A simple form of such further elaboration might be the classification of "final" goods into "fixed capital" and "consumption" goods, and the calculation of the proportions between these that would need to be maintained if the moving equilibrium was to be realised.

Progressive theoretical elaboration of this kind, if it were conducted in constant touch with "the facts," and if the progressive "categories" were wisely selected, might be expected to reach a stage at which the further indirect assumptions could be safely ignored, because the maintenance by social policy of the desired relations among the categories treated would (by virtue of the "strategic" nature of these) automatically ensure that

be necessary. But the truly scientific mind will never rest content simply with empirical knowledge of actually observed limits (or orders of magnitude, or of sequence), even when this knowledge extends over a long period, and when the arrangement of the individual instances in a frequency distribution exhibits a strong central tendency. Such things are but the raw materials of "science," in which it is never safe to assume that ultimates have been reached. The scientific economist will proceed to seek for further "necessary" conditions behind such limits and uniformities; and these again he will not be content to accept merely as empirical "brute facts."

Thus the true scientific method (in economics as in every other science) consists not in "presenting the totality of phenomena" (an impossible and unthinkable proceeding), and then somehow "extracting" empirical generalisations from the "facts" by a naïve process of *inductio per enumerationem*: it consists rather in the step-by-step coöperation of "induction" and "deduction" towards the ideal (but never completely attainable) goal in which both the totality of actual phenomena and their totality of necessary connections are displayed together in an integrated and harmonious whole.

the further underlying adjustments would follow. Further discussion of this suggestion, and of the precise nature of such "strategic" categories, would involve an inquiry into the basic nature and methods of social control; and this inquiry, if conducted systematically, would expose to view the intimate and organic relations existing between Positive Economics on the one hand and Social Philosophy (with its specialised department of Normative Economics) on the other. All that can be said here on this vast subject is that it would be hasty and unwise to assume that such "control" either should be, or *can* be, of a purely "mechanical" nature.

Nothing would be gained by optimistic prophecies regarding the future developments in economic science that the prosecution of such methods of investigation and analysis would ensure. The difficulties are formidable, and progress is not likely, perhaps, to be much more rapid than it has been along other lines in the past. But the methods indicated have at least the merit of embodying a balanced and acceptable conception of scientific methodology, as distinct from the one-sided outlook of a self-styled "empirico-realism," which apparently sees an insurmountable opposition between "logical necessity" and "empirical reality," and fails to see that the continuously fluctuating interrelationships of temporal processes are inherently just as amenable to treatment by "the equational method" as are cross-section instantaneous relationships. The suggested methods also appear to promise a maintenance of that essential continuity of development of economic science which, despite the vociferations of the various schools of irreconcilables in the past, History has always ultimately vindicated.

All good economists should therefore pray daily for

the preservation of their science from the excesses of an aggressive and militant "empirico-realism." But they should also remember, in their devotions, that the curious persistence in economics of this naïve philosophy of method can probably only be ultimately explained on the assumption that it thrives on the perhaps unavoidable human lapses of those whose zeal for applying scientific conclusions to the complexities of real life occasionally outruns their discretion.

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CREDIT EXPANSION, 1920 TO 1929, AND ITS LESSONS

SUMMARY

Credit Expansion. — Bank loans and investments, 95. — Urban real estate mortgages; held by banks, mortgage trusts, mutual savings banks, Life Insurance Companies, Building and Loan Associations, 96. — Farm mortgages; held by Federal Land banks, Joint Stock banks; general data, 105. — Securities outstanding, 107. — Installment selling; electrical equipment, radio industry, General Motors Acceptance Corporation, 108. — Summary, 115. — Credit expansion somewhat analogous to monetary inflation, 119. — Consequence of credit expansion: illustrated by developments in the radio industry, the automobile industry, the construction industry, 121. — Conclusion, 128.

In every acute period of depression we have a bewildering offering of explanations proferred as to the cause of our distress. As time elapses the fundamental mistakes in our industrial, financial, or commercial practises become clearly evident. The wisdom attained long after the event has slight value compared to its possible effects if apprehended at or before the period of crisis. Given early knowledge of the causes of business readjustment, those controlling industries might more promptly correct their mistaken policies, more wisely plan for future commitments, and more surely avoid repetition of disastrous errors. The thesis of this paper is that the existing depression was due essentially to the great wave of credit expansion in the past decade. There is a lamentable lack of comprehensive and accurate data concerning this process of debt creation. But highly suggestive information may be assembled regarding the growth of bank loans and investments; the increase in

mortgage indebtedness, urban and rural; the increasing volume of securities outstanding; and the expansion of installment credit.[1] This evidence will be discussed in turn.

The statistics of bank loans and investments show a

ALL BANKS[1] IN THE UNITED STATES — LOANS AND INVESTMENTS OF MEMBER AND NON-MEMBER BANKS, 1914–29

(In Millions of Dollars)

Date[2]	Loans and investments		
	All banks	Member banks[3]	Non-member banks[4]
1914 — June 30	$20,789	$8,313	$12,475
1915 — June 23	21,466	8,764	12,702
1916 — June 30	24,587	10,315	14,271
1917 — June 20	28,287	12,453	15,833
1918 — June 29	31,813	18,507	13,306
1919 — June 30	36,570	22,242	14,330
1920 — June 30	41,685	25,559	16,125
1921 — June 30	39,999	24,121	15,880
1922 — June 30	39,956	24,182	15,774
1923 — June 30	43,738	26,507	17,230
Dec. 31	44,003	26,487	17,516
1924 — June 30	45,180	27,167	18,013
Dec. 31	47,182	28,746	18,437
1925 — June 30	48,830	29,518	19,312
Dec. 31	50,603	30,884	19,720
1926 — June 30	51,562	31,184	20,378
Dec. 31	52.018	31,642	20,376
1927 — June 30	53,750	32,756	20,994
Dec. 31	55,450	34,247	21,204
1928 — June 30	57,265	35,061	22,204
Dec. 31	58,266	35,684	22,582
1929 — June 29	58,474	35,711	22,763
Dec. 31	58,417	35,934	22,483

[1] Includes member and non-member banks (25,110 altogether in June, 1929) as follows: National banks, state commercial banks and trust companies, mutual and stock savings banks, and all private banks under state supervision (about 265 in June, 1929).
[2] Dates of reports of member banks; figures for non-member banks are as of nearest available date.
[3] National banks.
[4] Non-national banks. 16th Annual Report, Federal Reserve Board, p. 101.

1. The writer makes grateful acknowledgement of generous assistance given him in his research for information by Miss Aryness Joy and Mr. F. R. Garfield of the Federal Reserve Board staff.

steady increase from 1922 to 1929, inclusive. This may be observed in the accompanying table. From the high point in 1920 of 41⅓ billions, the total of loans and investments held by all banks declined to just under 40 billions in 1921 and 1922. Thereafter this total began a steady upward climb. It rose over 4 billions in 1923, considerably over 3 billions in 1924, nearly 3.5 billions in 1925, lagged in 1926 at less than 1.5 billions, and resumed its upward march at the 3 billions per year rate in 1927 and 1928. Thus, in the six years after 1922, loans and investments held by banks had increased over 18 billions. This is over 45 per cent. Figures for all banks in 1929 show but slight increase over the total of loans and investments reported in 1928. In part this slight rise is a reflection of the stock exchange deflation in the fall of 1929. In larger measure it represents the ending of a period of credit inflation and of resultant business activity.

The great field of credit expansion in the last decade lies in the realm of urban real estate mortgages. There are several important holders of such mortgages. We may examine the holdings of commercial banks; the issue of mortgage bonds to be held by private investors; the investments in this class of loans by mutual savings banks; the holdings of Legal Reserve Life Insurance Companies and the expansion in the outstanding mortgage loans of Building and Loan Associations.

In 1927, as of June 30, an estimate was made of all non-farm mortgages held by certain classes of banks in the United States. This totaled $5.767,816,000. The table follows.[2]

2. See J. H. Gray and G. W. Terborgh, Trust mortgages in urban real estate finance. Brookings Institute, Washington, D. C., 1929, p. 4. The method by which the estimate was made is stated in footnote 7, p. 60.

NON-FARM MORTGAGES HELD BY BANKS, JUNE 30, 1927
(In Millions of Dollars)

Class of bank	Comptroller's figures	Our estimate
State Commercial Banks	$1,036.4	$2,050.0
Loan and Trust Companies	1,016.2	2,150.0
Stock Savings Banks	757.9	800.0
National Banks	757.3	757.3
Private Banks (partial returns)	7.8	10.5
	$3,575.6	$5,767.8

Similar figures for June 30, 1928, and June 29, 1929, in the Comptroller's report, show a total of $3,888,-205,000. and $3,246.789,000 respectively. Adding 60 per cent for omissions, as in the preceding estimate, makes a total of banking loans on non-farm real estate by these classes of banks in 1928 of much over 6 billions of dollars, and well over 5 billions in 1929.

NON-FARM MORTGAGES HELD BY BANKS
(In Millions of Dollars)

Class of bank	June 30, 1928	June 29, 1929
State Commercial Banks	$1,192.7	$ 912.2
Loan and Trust Companies	1,055.5	659.9
Stock Savings Banks	671.7	562.6
National Banks	960.8	1,104.2
Private Banks	7.3	7.7
Totals	$3,888.0	$3,246.6
Add 60 per cent for omissions	6,221.0	5,193.6

For the sake of a comparison, a similar computation is presented for earlier years.

On the same basis of a 60 per cent addition to the totals reported by the Comptroller of the Currency, the total of non-farm real estate mortgages held by these classes of banks was about 3.4 billion dollars in 1922, and 2.7 billion dollars in 1923. These are loose estimates but the assumption seems warranted that this class of

holdings by the banks included approximately doubled from 1922 to 1928, inclusive, and that their total of real estate loans is now well over 5 billions.

NON-FARM MORTGAGES HELD BY BANKS

(In Millions of Dollars)

Class of bank	June 30, 1922 [1]	June 30, 1923 [2]
State Commercial Banks	$967.8	$267.1
Loan and Trust Companies	474.1	629.5
Stock Savings Banks	573.6	750.2
National Banks	87.0	23.1
Private Banks	17.9	3.4
Totals	$2,120.4	$1,673.3
Add 60 per cent for omissions	$3,392.6	$2,677.3

[1] See Annual Report, Comptroller of Currency, 1922, pp. 120 et seq.
[2] Ibid., 1923.

Available information on mortgage bonds is not very satisfactory. The American Bond and Mortgage Company estimated the amount issued in each of the years 1919 to 1927. The Commercial and Financial Chronicle gives an annual compilation of financing. Under land, buildings, etc., the total issues reported for each year from 1920 to 1925, inclusive, were as stated in the following table:

REAL ESTATE SECURITY OFFERINGS

(In Millions of Dollars)

	Commercial and Financial Chronicle	American Bond and Mortgage Company
1920	$92.4 [1]	$77.4 [2]
1921	54.5	49.7
1922	178.5	160.0
1923	252,1	239.3
1924	335.5	322.4
1925	752.6	695.5

[1] I am indebted to M. W. D. Seibert of the Commercial and Financial Chronicle staff for these estimates.
[2] Gray and Terborgh, p. 6.

These totals include both long and short loan bonds and notes and stocks issued against lands and buildings.

S. W. Straus and Company have made compilations of "Real Estate Security Offerings, including mortgage bonds, debentures, collateral trust obligations, and land trust certificates publicly advertised or announced" for each of the years from 1926 to 1929, inclusive.[3] Their estimates, with comparison as before, were:

REAL ESTATE SECURITY OFFERINGS
(In Millions of Dollars)

Year	No. of issues	S. W. Straus and Company	American Bond and Mortgage Company
1926	1,083	$865.1	$906.0
1927	1,077	783.2	1,016.2
1928	923	833.0	
1929	492	395.1	

No one has ventured to make an estimate of the total volume of such securities outstanding and such an attempt is hazardous since it is probable that our information for the earlier years is very incomplete. We know, however, that reliance on this class of securities is comparatively recent and that the period of the bonds tends to be long.

Assuming that 500 millions were outstanding in 1920,[4] that one twentieth of such paper outstanding is retired each year, and accepting the figures presented in the first column as a substantially complete record of issues in each year of the decade, we would have the following table representing the volume of such paper outstanding:

3. This table was kindly furnished by Mr. W. C. Clark of the Straus organization.

4. Chamberlain and Edwards (Principles of Bond Investment, chap. 24, Real Estate Bonds) state that annual issues of these bonds had probably never exceeded 50 million before 1919. They put the issue at 500 million dollars in 1923 and over a billion in 1926. The practice of "conservative bond houses" is stated to be to arrange serial maturities to retire 35 to 50 per cent in from 10 to 15 years. Early issues carried from 6 to 8 per cent interest.

ESTIMATED VOLUME OF REAL ESTATE SECURITIES OUTSTANDING

(In Millions of Dollars)

Year	Amount outstanding at beginning of year	New issue	Amount outstanding at end of year	Amount retired
1920			$500.0	$25.0
1921	$475.0	$54.5	529.5	26.5
1922	503.0	178.5	681.5	34.1
1923	647.4	252.1	899.5	45.0
1924	854.5	335.5	1,190.0	59.5
1925	1,130.5	752.6	1,883.1	94.1
1926	1,789.0	865.1	2,654.1	132.7
1927	2,521.4	783.2	3,304.6	165.2
1928	3,139.4	833.0	3,972.4	198.6
1929	3,773.8	395.1	4,168.9	

No great emphasis is to be placed on the validity of the computation. The essential point illustrated is that in the use of these long term mortgage bonds a system of finance has been applied excellently well calculated to lead to credit inflation and this without violation of any of the accepted canons of credit security. Funds realized from the sale of such paper may be abundantly secured. Such funds flow into construction and result in business activity and seeming prosperity. The long term character of the paper, long postponement of the major portion of the principal, the gradual retirement of the balance, and the relatively low interest rate, combine to lengthen the period before the annual retirement of bonds falling due plus the interest payment equals the amounts realized from the sale of new paper. Meanwhile credit inflation proceeds unchecked. Inevitably the swelling volume of outstanding paper brings burdens which makes this inflation a self-limiting process. But its period is long and before its limits are reached a halt may be called by the absorption of all available investment funds.

It will be noted that the period of rapid expansion in this investment field is in the four years following 1924. In 1925, 1926, 1927, and 1928 the data presented show a total of nearly three and a quarter billions of real estate securities offered to the investing public. This is in antithetical contrast with the history of farm mortgages whose period of most rapid increase followed the depression in 1921. Some part of our boasted prosperity in recent years undoubtedly rested on nothing more permanent than the process of credit expansion — or inflation — illustrated above. It was most unfortunate for us that this process was substantially duplicated in other departments of our finances during the past decade.

It is probable that the Mutual Savings Banks held over 5 billions of non-farm real estate loans in 1928. Reports from 603 of the banks with 11,500,000 depositors were compiled in that year and gave a total of $4,868,748,300 of real estate loans. "The proportion of these loans secured by farm property is very small, the estimate of well-informed observers running below five per cent." [5] The list of banks reporting was incomplete, since the Comptroller of the Currency had data for 616 mutual savings banks in 1928 with 11,732,143 depositors. His division of loans is unsatisfactory due to the large amount reported not classified. In 1922 just under a billion dollars of non-farm real estate loans were reported by the Comptroller; in 1923, 2.75 billions; in 1928, slightly over two billions, and in 1929, but 683 millions. With such incomplete and erratic data, no conclusion is possible other than the assumption that real estate loans held by this class of banks probably

5. See Savings Bank Journal, July, 1928, p. 113. The reports secured were variously dated in 1927 and 1928.

have not greatly increased in the decade and that these holdings were near 5 billions in 1928.

The investments of Life Insurance Companies in real estate loans rise to imposing totals. Moreover, the increase in the last decade is very large. The accompanying table shows that these companies held a billion and one quarter of such investments in 1921, a total but little increased since 1916, when they held just under a billion of real estate loans. Three quarters of a billion were added to the total by 1924, and thereafter there was a steady increase in the totals reported by about a half-billion in each year up to and including 1929. The total of nearly 5 billions on December 31, 1929, is nearly four times as great as these holdings in 1921.

INVESTMENTS AND RESERVES OF 52 LEGAL RESERVE LIFE INSURANCE COMPANIES [1]

(In Millions of Dollars)

Date	Non-farm mortgages	
	Amount	Per cent
Dec. 31, 1906	$551,864	19.2
Dec. 31, 1911	$820,962	20.3
Dec. 31, 1916	992,333	18.5
Dec. 31, 1921	1,252,581	16.7
Dec. 31, 1924	2,019,972	20.9
Dec. 31, 1925	2,507,401	23.4
Dec. 31, 1926	3,152,978	26.5
Dec. 31, 1927	3,701,634	28.1
Dec. 31, 1928	4,290,583	29.2
Sep. 30, 1929	4,668,984	29.8
Dec. 31, 1929	4,831,000	29.9 [2]

[1] Proceedings of the Twenty-third Annual Convention of the Association of Life Insurance Presidents, p. 41. (These Companies held, in different years, from 91.6 to 98 per cent of the admitted assets of all United States Legal Reserve Companies.)
[2] Estimated by Association of Life Insurance Presidents.

Similarly rapid is the growth of the holdings of mortgage loans by the Building and Loan Associations. And the total held is even more imposing than that of the Life Insurance Companies. Reference to the table shows that the Building and Loan Associations had assets in

1928 of over 8 billion dollars. Of these assets, 91.5 per cent or 7⅓ billions, were mortgage loans. In the years for which data are available the mortgage loans make about 92 per cent of the total assets. It is safe to assume that this was near the truth in 1920. We may, therefore, estmate the mortgage loans held in that year at about 2½ billions. This means that the growth in the total value of real estate loans held by Building and Loan Associations was over threefold in the years from 1920 to 1928, inclusive. The year just passed failed to maintain the average of the four preceding years. However it added another 450 millions to this impressive total of mortgage loans.

BUILDING AND LOAN ASSOCIATIONS[1]

(In Millions of Dollars)

Year	Assets	Increase	Mortgage loans	
			Per cent	Amount
1895	$580			
1900	571	$ −8		
1905	629	58		
1910	932	303		
1915	1,484	532		
1920	2,520	1,036	(92.)[2]	$2,318
1921	2,891	371	(92.)	2,659
1922	3,343	548	(92.)	3,065
1923	3,943	600	(92.)	3,627
1924	4,766	823	(92.)	4,384
1925	5,509	743	92.3	5,085
1926	6,334	825	92.4	5,852
1927	7,179	845	91.7	6,583
1928	8,016	837	91.5	7,336
1929	(8,695)	(679)	(89.6)	7,787

[1] Annual Reports of United States League of Local Building and Loan Associations.
[2] Bracketed figures are estimates; others as stated in the official reports.

Combining these holdings of various investors in urban mortgage loans with interpolation and estimates to fill gaps in the data, gives the table next presented.

HOLDINGS OF URBAN REAL ESTATE MORTGAGES

(In Millions of Dollars)

Year	Commercial banks	Mortgage bonds	Mutual savings banks	Life insurance companies	Building & loan ass'ns	Totals
1920.....	$(3,000)[1]	$5,00.0	$(4,000)	$1,252	$2,318[2]	$11,070.0
1921.....	(3,000)	529.5	(4,125)	(1,443)	2,659	11,756.5
1922.....	(3,393)	681.5	(4,250)	(1,634)	3,065	13,023.5
1923.....	(2,677)	899.5	(4,375)	(1,825)	3,627	13,403.5
1924.....	(3,449)	1,190.0	(4,500)	2,019	4,384	15,542.0
1925.....	(4,221)	1,883.1	(4,625)	2,507	5,085	18,321.1
1926.....	(4,993)	2,654.1	(4,750)	3,153	5,827	21,377.1
1927.....	5,767	3,304.6	(4,875)	3,701	6,583	24,230.6
1928.....	6,221	3,972.4	(5,000)	4,290	7,336	26,819.4
1929.....	5,195	4,168.9	(5,125)	4,831	(7,787)	27,106.9

[1] Bracketed figures interpolated or estimated.
[2] 92 per cent of total assets. This is based on statements of percentage mortgage loans and total assets for the years 1925 to 1928, inclusive.

It must be remembered that this table includes, for the most part, only first-mortgage obligations. The carriers included rarely accept junior lien paper. No emphasis need be placed on the individual items in the estimate The essential point is that indebtedness of this character, accepting the totals as they stand, increased from 11 to 27 billions of dollars in ten years. This is nearly 150 per cent increase. The absolute gain of 16 billions overtops by 5 billions the entire debt of this character amassed in all the earlier years of our urban development. It should be noted that the period of most rapid increase lies in the three years 1925, 1926, and 1927. Each of these years shows roughly 3 billions of new urban real estate debt. Together the three years account for nearly 9 billions of the total increase of nearly 16 billions. Comparison of the billions of increased indebtedness piled up in the decade with the total of real estate debt in 1920 will emphasize the significance of this development. We have undoubtedly greatly expanded the credit structure, spending today

and postponing the accounting until tomorrow. We have been guilty of the sin of inflation. And there will be no condoning the sin nor reduction of the penalty because the inflation is of credit rather than a monetary one.

To this data for urban mortgage loan expansion may be added evidence of an increased debt burden imposed on farm lands. The Federal Land Banks are authorized to write mortgages with a time period of from five to forty years. In practise these mortgages have a time limit of thirty-six years, that being the period of amortization when 1 per cent of the original principal is paid yearly. In connection with the rapid increase in farm loans by this agency, it should be noted that this lengthening of the loan period from the normal five years' span has operated to increase the total outstanding in any given year. The earliest report of the Federal Land Banks, in 1918, shows a total of 150 millions of mortgage loans outstanding. This had increased to over a billion by 1925 and reached $1,197,949,000 in 1929. This increase was most rapid in the years between 1921 and 1925. The annual increases in those three years aggregated just under 500 million dollars. The detailed table is given below.

The system of Joint Stock Land Banks was built up in the same period as the Federal Land Banks, and their mortgage loans are of the same general type. In 1918 banks of this type reported a total of 8.4 millions of mortgage loans outstanding. By 1925 this total had been increased to well over 500 millions and by the close of 1927 it had reached $669,798,000. The two following years show a decrease. There were 48 active Joint Stock Land Banks in 1929. The period of most rapid increase coincides very closely with that of the Federal Land Banks. The three years 1922, 1923, and 1924, show over

12 FEDERAL LAND BANKS; TOTAL MORTGAGE LOANS OUTSTANDING, AND ANNUAL INCREASE [1]

(In Millions of Dollars)

Year	Total loans outstanding	Increase
1918	$156.2	
1919	293.6	$137.3
1920	349.6	56.0
1921	432.5	82.8
1922	639.5	206.9
1923	799.6	160.1
1924	927.5	127.9
1925	1,005.7	78.1
1926	1,077.8	72.1
1927	1,155.6	77.8
1928	1,194.4	38.8
1929	1,197.9	3.4

[1] See Thirteenth Annual Report of the Federal Farm Loan Board, 1929, p. 33.

360 millions of increased loans. The report by years is as follows:

JOINT STOCK LAND BANKS; TOTAL MORTGAGE LOANS OUTSTANDING, AND ANNUAL INCREASE [1]

(In Millions of Dollars)

Year	Total loans outstanding	Increase	Decrease
1918	$ 8.4		
1919	60.0	$51.6	
1920	77.9	17.9	
1921	85.0	7.0	
1922	218.7	133.7	
1923	392.6	173.8	
1924	446.4	53.7	
1925	545.5	99.1	
1926	632.4	86.9	
1927	669.8	37.3	
1928	656.5	...	$13.2
1929	626.9	...	29.5

[1] See Thirteenth Annual Report of the Federal Farm Loan Board, 1929, p. 64.

The statistics of farm mortgage loans held by these Federal agencies give an exaggerated impression of the expansion of total farm indebtedness. The Federal

Land Banks in large part have been taking over the loans previously held by commercial banks and by private lenders. The available information regarding the total of farm mortgages outstanding is based, for the most part, on census returns. It is more comprehensive in the years covered than the data presented above for urban realty. All mortgages, including second liens, are covered, and all farms were canvassed. The census of 1920 [6] showed a total of 7,857.7 millions in mortgage debt on United States farms. By the close of 1924 this total had increased to 9,360.6 millions, and by the close of 1927 to 9,486.5 millions. Indications for 1928 and 1929 [7] are that the total farm indebtedness has not greatly increased. It may even prove to have declined. Of the principal holders, Federal Land Banks, Joint Stock Land Banks, and the Insurance Companies, only the first showed increased holdings in 1928. The net rise was only 4 millions. In contrast the Joint Stock Banks showed a decline of 21 millions, and the Insurance Companies a fall of about 48 millions. A general statement for the decade shows a rise of 1.5 billions in the first four years, and a very gradual rise of less than one half billion in the following five years. This slackening of the rate of increase has been accompanied by a rise in the interest rate. The rate charged varies with different agencies from 5.5 to 6.6 percent.[8] At 6 per cent the annual interest charge foots up nearly 600 million dollars. To this must be added a percentage for amortization payments on the principal.

Further suggestion of the increasing credit inflation is found in Moody's estimates of the total volume of

6. See the article by David L. Wickens in Year Book of Agriculture, 1930, pp. 389 et seq.

7. See the article by the same author in the Fifth District Banker and Financier, March, 1930.

8. Ibid.

Securities Outstanding.[9] The estimates are in billions of dollars.

Year	Corporate bonds and notes	Public securities	United States bonds	Totals
1920	$26.1	$11.8	$24.0	$61.9
1923	31.6	17.6	22.0	71.2
1924	34.1	20.0	20.9	75.0
1925	37.0	23.4	19.9	80.3
1926	40.2	25.8	17.0	83.0
1927	44.1	28.4	17.7	90.2
1928	47.1	33.6	18.9	99.6

Corporate bonds and notes represent net debt increase. They show a rise from 26 billion in 1920 to 47 billions in 1928. This is over 20 billions of increased credit in eight years. Public securities in the same period nearly trebled in volume outstanding. Less than 12 billions were out in 1920; over 33.5 billions in 1928. This means an absolute increase of well over 20 billion dollars. United States bonds showed a slight decrease. The total volume of such securities estimated as outstanding rose from about 61.9 billions in 1920 to 99.6 billions in 1928.

The expansion of credit involved in installment selling has been hotly and voluminously debated. This sales method has been variously hailed as the foundation of our prosperity and as the most dangerous credit development of this decade. Our information is lamentably incomplete, but there is clear evidence of its effect on the inflation of credit. In older fields the lengthy period of development and the relatively inelastic character of the well-established demand for the commodities thus sold has resulted in a condition of equilibrium. New debts created are matched by installments paid and there is no net result in credit expansion. This is the

9. See the Annual Volumes on Industrials: Introduction, Nation's Basic Industries. These estimates have been discontinued and data for 1929 are not furnished.

result to be anticipated in fully developed markets. It is probably the condition in the current installment sales of pianos, foot-power sewing machines and jewelry. They have established markets and a stable clientele. The annual volume of sales in normal years probably does not vary widely.

Even in these least significant markets, from the standpoint of credit expansion, there is room for consideration of the net effect of our nation-wide acceptance of installment practises. Rural districts and small cities are quite as familiar with down payments and monthly installments as are the largest cities. All income classes up to the richest have succumbed to the allurements of easy possession and "pay as you earn." And one commodity after another has entered the competition to secure sales by offering easy terms of possession and payment. There is scant room to doubt, after scanning the advertisements of our popular magazines, that the area of installment sales is still increasing and with it the volume of such credit outstanding.

The furniture industry well illustrates an old field in which installment sales have greatly increased. Recent studies estimate that 70 to 80 per cent of furniture is now sold on installments.[1] And inquiry as to credit possibilities in any store selling furniture will suggest that this estimate is justified. Clothing stores have likewise succumbed. It has been estimated[2] that 140 million dollars worth of clothing is sold on installments annually. The common period of credit is ten weeks and the volume of debt outstanding at any time is, therefore, less than the total so sold might suggest. Never-

1. See Recent Economic Changes, i, 393.
2. William C. Plumer, Social and Economic Consequences of Buying on the Installment Plan, Supp. vol. cxxix, Annals of American Academy, 1927, p. 52.

theless, the amount outstanding is considerable, being put at 40 millions.

It is in the case of the newly invented commodities with a virgin market and an enormous annual increase in volume of sales that installment selling fully demonstrates its capacity for credit expansion. Here the process may be fitly termed inflation. Electric equipment such as refrigerators and washing machines, and the radio, well illustrate the process. The following table is compiled from data presented in Electrical Merchandising. It is to be regarded as a suggestive sample rather than a comprehensive showing.

The reader will note the absence of data for the years 1924 and 1925. The presentation well illustrates the contrast between the marketing results of the different commodities. Electric irons have a practically stabilized market both in units sold and their value. Vacuum cleaners and sewing machines show a gradual increase in units sold with a tendency to stability in their total value. Ironing machines show a steady increase in the number of units sold with a parallel increase, though less rapid, in their value. The number of washing machines sold in 1929, as contrasted with 1927, was nearly 25 per cent greater, rising from 775,000 to over 1,000,000. But the total value of the million machines sold in 1929 exceeded that of the 775,000 units sold in 1927 by less than 5 per cent. This exemplifies the normal result as a market approaches maturity with potential productive capacity well up to possible sales, with the more eager buyers supplied and with increasingly intense competition for the current business.

Electric refrigerators have been placed last as a typical example of the triumphant progress of a commodity with an untouched market, an enormous field, and, it may be added, a whole-hearted acceptance of the in-

Units of Electrical Equipment Sold

	1922	1923	1926	1927	1928	1929
Vacuum Cleaners	800,000	1,025,000	1,065,000	1,194,614	1,219,460	1,312,000
Electric Irons	3,300,000	2,250,000	3,000,000	3,000,000	3,000,000	3,150,000
Ironing Machines	50,000	50,000	57,000	68,000	92,000	126,000
Washing Machines	415,000	565,000	843,000	775,661	809,884	1,019,000
Sewing Machines	140,000	625,000[1]	315,000	350,000	380,000	399,000
Electric Refrigerators	11,000	15,000	248,000	365,000	468,000	630,000

Value of Electrical Equipment Sold
(In Millions of Dollars)

Vaccum Cleaners	$40.0	$56.0	$65.0	$58.5	$60.9	$65.6
Electric Irons	20.0	13.4	16.0	14.2	13.7	14.4
Ironing Machines	6.0	6.0	8.8	10.8	8.5	9.6
Washing Machines	65.0	70.0	124.0	110.9	108.0	115.0
Sewing Machines	10.0	37.7	29.9	33.2	35.0	36.7
Electric Refrigerators	4.0	5.3	65.2	82.1	128.9	181.1
Total	$145.0	$188.4	$308.9	$309.7	$354.8	$422.4

[1] This figure seems doubtful.

stallment plan for sales. The years presented cover practically the entire history of this commodity. Eleven thousands units were reported sold in 1922, and 15,000 in 1923. By 1926, however, the industry was established and quantity methods of production and selling had been organized. Nearly a quarter of a million units were sold in that year, with an estimated value of over 65 million dollars. This record had increased to 630,000 units sold by 1929, with an estimated value of over 180 million dollars. Installment selling with a modest sum down and a lengthy period for completing the payments has been the accepted practise in this industry. It is instructive to note the resultant credit inflation consequent on such practise in the case of a new and rapidly expanding industry. The current statement is that 75 per cent of all sales are on a credit basis. Applying this percentage to the estimated value of sales we get the following showing as to the increase of credit:

Year	Value of units sold	Credit sales	Down payments
	(In thousands of dollars)		
1922	$4,000	$3,000	$1,000
1923	5,300	3,875	1,425
1926	65,200	48,900	16,300
1927	82,125	61,594	20,531
1928	128,700	96,525	32,175
1929	181,175	135,881	45,294

Thus, the value of electric refrigerators sold increased from 65 millions in 1926 to over 180 millions in 1929, but the initial payments in 1929 were much less than the total value of the units sold in 1926, while the volume of debt annually created rose from 3 millions in 1922 to nearly 136 millions in 1929.

A similar illustration of credit expansion may be drawn from the radio industry. It is estimated[3] that no

3. See Radio Retailing for January, 1930, p. 23.

more than 60,000 homes had radio sets in 1921. The amazing history of this newly created industry is statistically summarized in the table following:

TOTAL RADIO SALES[1] (1922–28)

Year	No. sets sold (In thousands)	Value of sales (in millions of dollars) *Sets*	*Parts*	*Accessories*	*Totals*
1922	100	$5	$40	$15	$60
1923	250	15	75	46	136
1924	1,500	100	100	158	358
1925	2,000	165	65	200	430
1926	1,750	200	50	256	506
1927	1,350	168	21	236	425
1928	2,550	388[2]	12	290	690
1929[3]	4,200	525	7	248	842

[1] Reprinted from Radio Retailing for January, 1929. Figures for 1929, in March 1930 issue, p. 41, and revision of the 1928 estimates are found in the Radio Retailer for January, 1930, p. 23.
[2] Includes "Combinations."
[3] A statistical survey of the Radio Industry.

Applying the 75 per cent standard for credit selling to the value of sets sold, disregarding the very considerable sums realized from the sale of parts and accessories, we get the following example of credit inflation:

[1] Year	Value of sets sold[1]	Credit at 75 per cent (In millions of dollars)	Down payments
1922	$5	$3	$1
1923	15	11	3
1924	100	75	25
1925	165	123	41
1926	200	150	50
1927	168	126	42
1928	388	291	97
1929	549	412	137

[1] A Statistical Survey of the Radio Industry — Reprinted from Radio Retailing, January, 1929. Figuresfor 1929 in March, 1930, issue, and revision of 1928 estimates are found in the Radio Retailer for January, 1930, p. 23.

The table given below (p. 114) illustrates the extraordinary growth of the industry.

Thus, this astonishing growth from sales of sets worth 5 millions in 1922 to sales aggregating well over a half-billion in 1929 brings with it an increase in debt of well over 400 millions in this short space of seven years. To

the extent of 550 millions of dollars the radio industry has induced business activity in 1929. It is probably well within bounds to conclude that this activity and temporary prosperity has been attained by drawing drafts on the future to the extent of well over 400 millions of dollars. Obviously, this is a self-limiting process. Its very rapidity and magnitude insures a speedy check and a thorogoing process of readjustment.

NUMBER OF HOMES WITH SETS [1]

Dec. 31, 1921	60,000	Allowing for replacements and obsolete sets.
1922	1,500,000	
1923	3,000,000	
1923	4,000,000	
1925	5,000,000	
1926	6,500,000	
1927	7,500,000	
1928	9,500,000	
1929	11,500,000	

[1] See Radio Retailing for January, 1930, p. 23.

Corroborative evidence of the examples just adduced is found in the records of such finance organizations as the General Motors Acceptance Corporation. This company was formed in 1919 as a subsidiary of General Motors. Its function was to handle the paper arising from that company's installment sales. In the year of the formation of this acceptance corporation, General Motors acquired Frigidaire. Notes receivable held by this corporation increased as shown below:

GENERAL MOTORS ACCEPTANCE CORPORATION [1]

(In Millions of Dollars)

Year	Notes receivable	Year	Notes receivable
1920	$25.7	1925	104.5
1921	26.2	1926	218.7
1922	45.7	1927	275.7
1923	67.3	1928	322.6
1924	55.9	1929	400.8

[1] Data taken from Moody's Industrials.

Thus, a single corporation, marketing newly introduced commodities on the installment plan, accounts for debt creation to the extent of 400 millions of dollars in ten years. The bulk of this impressive total, 300 of 400 millions, was rolled up in the last five years of the period covered by the table.

Without further elaboration we may accept the current estimates that annual installment sales are now about 6 billions [4] and that the total debt outstanding at a given time is about half that sum, or 3 billions. Of this debt about half results from the sale of automobiles and trucks, both new and in the used car market. The factor of importance here is that this imposing total of 3 billions of installment indebtedness is a result of recent expansion. That plan of selling originated in the automobile industry about 1920.[5] Its period of rapid expansion in that industry was in the next five years and was associated with the rapid growth of finance companies. The zenith of that development was put by a competent authority [6] at the beginning of 1925 when 1000 to 1500 companies were operating. This is therefore new indebtedness. It represents credit inflation and its influence is not limited to the fields covered by statistics of installment selling. There is competition between industries. Old line dealers handling established lines perforce compete with new companies handling newly invented commodities and enthusiastically promulgating the installment sale philosophy. The older dealer yields of necessity to the pressure of this competition. He can only maintain his place in the business world and retain his hold on the consumers' income by offering equally tempting credit terms. Thus the area covered by credit

4. See Plummer, ut supra, pp. 2 et seq.
5. E. R. A. Seligman, Installment Selling, i, 22 et seq.
6. C. C. Hanch, writing in American Bankers Association Journal.

sales enlarges and the volume of credit expansion increases. As in monetary inflation the immediate results seem favorable. Credit expansion results in business activity, in full employment, in optimistic outlook and in a flood of gratulatory literature proclaiming us wiser than our predecessors.

The evidence as to debt expansion here presented is fragmentary. It is often incomplete. We have no data as to junior liens on urban real estate, which, in the aggregate, amount to enormous sums. There is no record of loans extended by individuals or of personal loans to small borrowers. Book credit, estimated to amount to from 20 to 30 billions of dollars, was probably much expanded in the decade, but a statistical record is lackng. In the field of installment selling the current estimate of a total of 3 billions outstanding may be very wide of the mark. The reports of the various carriers of our debt burdens often overlap. Thus the banks carry much mortgage indebtedness both urban and rural. The securities outstanding include bonds based on real estate security, and many securities are included in the invesments of banks. Bank loans may be a feature of security issues or based on collateral resulting from installment sales. No general total may be attempted. But the evidence is consistent and cumulative. The past decade has witnessed a great volume of credit inflation. Our period of prosperity in part was based on nothing more substantial than debt expansion.

Several financial devices of recent invention have contributed to this process of debt inflation. In the long run their net results may prove beneficial. For the moment the resulting credit inflation was undoubtedly mischievous. The Federal and Joint Stock Land Banks refinanced a growing proportion — now approximately one fifth — of rural land mortgages into long term paper.

This gave the borrowers security. They were freed from the necessity of frequent refinancing, assured of easy payments and fair treatment. Their loans do not fall due in times of financial stringency and high interest rates. Instead they are annually reduced and finally extinguished in orderly fashion. But this process, while desirable and helpful when an equilibrium has been reached, all worthy borrowers being accommodated and the volume of interest and amortization payments furnishing the funds needed for new loans, results during its period of introduction in expanding the volume of indebtedness. Instead of a determined struggle to retire the mortgage in five, seven, or ten years, the poorer families are tempted to make the easier installment payments and indulge their desire for consumption goods with the income freed. Granting that under the previous practice not many farm mortgages were fully paid in five years, is it not also true that very few were carried through thirty-six years? In the early years of the adoption of this lengthier method of farm debt payment the result to be anticipated is an increase in the volume of farm mortgages. Some part of the billion and a half of new rural debt piled up in a decade of shrinkage in land values and in rural population must be charged to this scheme for long time credit.

Of similar tendency but more obvious in its recent developments is the newly originated and rapidly introduced device of urban real estate bonds. As a method of credit inflation this plan could hardly have been bettered had its progenitors consciously framed its provisions with that end in view. Its period is ten years or more. The bulk of the principal falls due at the end of the period. No more than 35 to 50 per cent is paid in the first ten or fifteen years. There has thus resulted great ease in flotation in the decade just closing, seemingly full

success in meeting the terms of the bonds, and, in the first half of the period at least, full security based on genuine scarcity of buildings, high rents, and consequent enhanced values. The volume successfully sold rolled up with the speed of the proverbial snowball travelling down a steep hill. The fruits of such sales gave us building activity and contributed to the flush times of the decade. In residential building we find a parallel development in the rapid expansion of building and loan associations with their monthly payment plans. The reckoning was postponed to the future.

The fundamental question regarding installment sales is of this sort. The method *per se* is unexceptional. Granting that sound credit principles are applied such sales are safe. But it is highly probable that a considerable volume of the sales recently made were based on credit ratings only justifiable on the theory that flush times were to continue indefinitely. The competition for the consumers' dollar became stiff — not to say furious. These, however, are the sort of mistakes which long run experience combs out. It so happened that this new credit method coincided in the time of its introduction with the origination of new consumption goods of the widest popular appeal. Its enthusiastic adoption in the marketing of these new commodities as well as the simultaneous development of new and very effective advertising organizations and methods have resulted in the very rapid increase of outstanding consumer credit. The round number total of 3 billion is probably a highly conservative estimate. This process of debt inflation went on apace.

We are urged to install delicately tinted bathrooms on the installment plan. We may have oil-burning furnaces on easy payments. Roofs may be replaced, houses

painted, and plants supplied with machinery, all on the basis of pay as we earn. This may be desirable, but the hard fact remains that such policies have resulted in a great expansion of indebtedness. Temporarily we have spent, enjoyed and stimulated business activity. In the long run we must repay. When the process of expanding credit ceases and we return to a normal basis of spending each year no more than we earn that year, there must ensue a painful period of readjustment. Producers have been selling a volume of goods equal in value to the total national income plus 5 or 10 per cent of new debt created. Sometime, and at no long distant date, they must return to the less spectacular but sounder basis of selling a volume annually equal to the national income in value. With all our Yankee inventiveness we may never hope to find a way to spend continuously more than we earn. For a few years the nation, like the individual debtor, may buy more than it earns, paying for the uncovered balance by increasing its indebtedness. Once the limit of its credit is reached the nation's purchasing power falls back to the limit of the national income. If the debts piled up prove insecure, annual purchases may shrink more than is thus suggested. Losses through bad debts and unwise investment must be written off.

Temporary stimulation of business activity through credit expansion, with a following check to prosperity followed by depression, has some analogies to the familiar round of events following upon monetary inflation. There is the same stimulus to demand; the same ability to purchase for a time an abnormal volume of goods. There is the same temporary prosperity. And judging by the literature produced in the last few years, there is apt to be the same flamboyant boasting of our

superior wisdom. Both processes end in depression while we pay up arrears and straighten out our industrial investment and organization distorted by the impacts of artificially created demands. But the monetary inflation reaches its end when rising prices absorb the excess currency and the nation's business comes back to an even keel at a higher price level. Debt inflation, on the contrary, need not spell enhancement of prices, except as activity in business and ease in making sales conduce to that result. Aside from debts represented by expansion of bank deposits, the new indebtedness does not enter the monetary circulation of the country. The end of the inflationary period comes when the new credit methods have worked out their full results. When every potential debtor and installment buyer has assumed the full burden of indebtedness which the new credit policies allow; when every would-be home owning family has purchased through the building and loan associations as costly a house as its resources will permit; and when every aparment house and business building has been burdened with as heavy a load of bonded indebtedness as the avid savers and investors can be persuaded to accept — in short when the newly tapped credit resources have been fully exploited — there is, of necessity an end to the process. While credit expansion continues we can produce, sell, and enjoy as much more than the annual money income as is covered by the increased debt burden. When expansion stops we snap back to the volume of goods covered by the national income.

This check is painful, and often aggravated in its results by mistakes made by the producers of goods. During the period of expansion and particularly in newly developed fields, the producer sees the volume of sales annually expanding. All that he can produce he sells at

very profitable rates.[7] Every consideration combines to urge him to expand his productive capacity as rapidly as possible to exploit his rich opportunities to the full. Under competitive conditions it is inevitable that the competing producers spurring on in their eager pursuit of profits should overshoot the mark. When the check comes they find themselves with a great excess of productive capacity. Much investment funds are hopelessly sunk in idle plants.

This sequence of events is aptly illustrated from the recent history of the radio industry. The table presented earlier in this article shows 2,000,000 sets sold in 1925 and over 4,000,000 in 1929. Despite this enormous expansion in sales there was warning in October of 1929 that the industry was faced by serious overproduction.[8] In that month there had been 336,000 sets on hand, an average of 8.6 sets per dealer. In the same month of 1929 nearly 569,000 sets were in stock. This was an average of over 14 sets per dealer. We are told that after the stock market break the retail dealer "practically stopped buying." Despite this disastrous close to the year, the total sales reached the enormous total stated above.

The sets manufactured in 1929 numbered 5 million, of which 900,000 represented overproduction. Factory capacity had been increased in 1929 from ability to produce about 4 million sets to equipment and plant capable of producing 15 million sets. The results of a sur-

7. Moody's Industrials gives the following statement of capital invested and annual profits in the automobile industry:

Year	Capital invested	Earnings Per cent
1919	$1.3 B's	21.8
1920	1.4 "	15.5
1925	1.8 "	20.6

8. Edgar H. Filer, Radio Consultant, National Electrical Manufacturing Association, writing in Electrical Merchandising, January 1, 1930, p. 69.

vey made by the recognized trade journal[9] "early in the summer" are too striking and too typical of the results of competition in a richly profitable field to be omitted here. Manufacturers were asked: "By what per percentage are you increasing your 1929 production above 1928?" The answers of 28 manufacturers follow:

	Per cent		Per cent		Per cent		Per cent
1	300 to 400	8	500	15	20	22	Orders
2	400	9	35	16	100	23	50
3	200	10	None	17	50	24	200 to 300
4	50	11	Orders	18	300	25	500
5	40	12	100	19	1,000	26	60
6	200	13	100	20	100	27	600
7	25	14	None	21	75	28	100

The radio industry at the end of the year was in sharp distress. Factories closed or went on part time. The surplus of sets in stock was sold at bargain prices. Thereafter production was resumed at a soberer rate. The sequence of events in this new industry points unerringly to the essential course of the difficulties encountered. It marketed a new, most attractive, and continuously improved commodity. There was no lack of enterprise, of productive capacity, of production, or sales initiative and ingenuity. Their public was "radio conscious" to an extraordinary degree. There were 17,500,000 homes without radio sets at the close of 1929. Terms of sale were made very attractive. The industry had enjoyed ten years of unbroken and very rapid progress and prosperity. Yet in full career it met a decisive check. With all its attractiveness and despite its liberal terms of sale, it had exhausted its credit line. Producers, financial backers, dealers, purchasers, and would-be purchasers had expanded their debt burden to the full amounts they were either able or willing to assume.

9. See Radio Retailing for December, 1929, pp. 30, 31.

Further debt expansion, if not impossible, was only possible at a decidedly slower rate of expansion. The full results of the policy of debt expansion had been enjoyed.

The recent history of the automobile industry is closely analogous to that of the radio industry. The production of motor vehicles has had a checkered career. Less than 5,000 passenger cars were produced in 1900. The first trucks, 411, are reported in 1904. Progress was steady and rapid until 1916, when 1,617,000 cars and trucks were produced. A decided check was experienced in 1918, followed by recovery in 1919. Statistics of production are presented for the last ten years:

PRODUCTION OF MOTOR VEHICLES [1]

Year	Passenger cars	Trucks	Total
1920	1,905,560	321,789	2,227,349
1921	1,529,165	153,200	1,682,365
1922	2,397,827	248,402	2,646,229
1923	3,780,358	400,092	4,180,450
1924	3,327,770	410,016	3,757,786
1925	3,904,566	523,234	4,427,800
1926	3,984,018	521,643	4,505,661
1927	3,093,428	486,952	3,580,380
1928	4,024,590	576,540	4,601,130
1929	5,651,000	805,000	6,456,000

[1] National Automobile Chamber of Commerce: Facts and Figures of the Automobile Industry, p. 6. Data for 1929 furnished by the Secretary.

Drops in the number of cars produced will be noted in 1924 and 1927. But 1928 showed an increased production of over a million motor vehicles and 1929 carried the year's increase to 1,850,000. The production of 1929 is most instructive when presented by months in comparison with 1928. The total number of cars and trucks produced with the percentage of monthly gain or loss was as follows.[1]

1. Automobile Trade Journal and Motor Age, February, 1930, pp. 56–57.

CARS AND TRUCKS PRODUCED

Month	1928	1929	Per cent gain or loss
Jan.	240,191	422,538	Gain 76
Feb.	336,300	497,640	" 48
Mar.	430,783	625,844	" 45
Apr.	434,315	663,248	" 53
May	459,725	635,643	" 38
June	425,195	566,848	" 33
July	417,312	517,853	" 24
Aug.	492,543	512,589	" 4
Sept.	436,507	429,514	Loss 2
Oct.	415,820	394,543	" 5
Nov.	268,909	226,887	" 16
Dec.	243,541	131,862[1]	" 46
Totals	4,601,141	5,625,000	Gain 22

[1] Partly estimated. Revised figures 125,489.

Just when the stoppage occurred is plainly indicated by the index of new car stocks. This stood at about 115 on January 1, 1929. This was on the basis of the monthly average of 1927 equalling 100. This index rose to over 125 by February first, and shot up to 170 by March first. It rose still further to 190 by April first. Thereafter there was a very gradual decline. It was still abnormally high at the end of the year.

The parallelism between developments in 1929 in the automobile and radio industries could hardly be more exact. In each case an expanded volume of goods was offered to a public highly desirous of purchasing them. In each industry a highly trained sales force exhausted every device known to high pressure salesmanship to sell the accumulating stocks. And in each case they found it impossible to move the goods, primarily because the buyers resources in cash and credit had been expended. The effect of the great bulge in credit during the decade had been exploited. For the future, the automobile industry, and all others, must be content with a normal annual volume of sales.

Strictly analogous in the principles involved and portentous in their magnitude were the developments in the construction industry. The main facts in the general progress of that industry in the last ten years are well known. The war put a stop to all building except that essential for war needs. We emerged from the world war with a genuine scarcity of all types of buildings. We suffered through the depression of 1921 and embarked on a period of flush years, the fruits of our credit expansion policy. In these flush years construction played a leading rôle and enjoyed great prosperity. This is best shown by the annual statistics of construction.

CONSTRUCTION CONTRACTS AWARDED IN 37 STATES [1]
(In Millions of Dollars)

1923	$3,981.3
1924	4,485.8
1925	5,822.2
1926	6,152.0
1927	6,303.0
1928	6,628.0
1929	5,754.2

[1] Source F. W. Dodge Corporation. Quoted in Recent Economic Changes, i, 220, table.

It is estimated [2] that construction in the remaining states, and the addition of small buildings and of some types of construction not covered, would carry the total for the three years 1926 to 1928 inclusive to more than 7 billion dollars. The monthly records for 1927, 1928, and 1929 show clearly just when the check to building operations occurred.

In the statistics of the general construction industry the early months of 1929 continue the story of expanding operations, but the last five months of the year show a distinct and increasingly rapid decline in the volume of contracts awarded, while the monthly record of

2. Automobile Trade Journal and Motor Age, 1930, note p. 219.

residential contracts shows clearly that the check came in this branch of building operation and that it was plainly marked with the turn of the year from 1928 to 1929.

The story as told by construction's leading authority

TOTAL CONSTRUCTION CONTRACTS (37 STATES)
(In Millions of Dollars)

	1927	1928	1929
Jan.	$384.4	$427.1	$409.9
Feb.	393.5	465.3	361.2
Mar.	620.7	592.5	484.5
Apr.	604.3	642.2	642.0
May	552.3	667.0	587.7
June	632.4	650.4	529.8
July	534.3	583.4	652.4
Aug.	552.4	516.9	488.8
Sept.	521.6	581.6	444.4
Oct.	562.8	597.1	445.6
Nov.	466.3	471.4	391.0
Dec.	477.3	432.7	316.3
	$6,303.0	$6,628.0	$5,754.2

RESIDENTIAL CONSTRUCTION CONTRACTS (37 STATES)
(In Millions of Dollars)

	1927	1928	1929
Jan.	$167.8	$193.1	$138.0
Feb.	163.0	238.9	129.4
Mar.	250.0	275.1	196.9
Apr.	267.4	276.5	256.7
May	219.9	288.8	192.0
June	239.8	258.7	173.8
July	186.9	228.7	199.9
Aug.	209.4	213.7	146.0
Sept.	202.8	196.8	117.3
Oct.	243.5	239.6	137.6
Nov.	214.9	200.2	113.5
Dec.	207.2	178.3	114.0
	$2,572.6	$2,788.4	$1,915.1

might have been written to the same outline as that presented above for the radio and automobile industries.[3]

"Construction contracts recorded during the year 1929 in the 37 eastern states reached a total of $5,754,290,500. Compared with the total for the record year 1928, this was a decrease of 13 per cent. Commercial and industrial building reached a combined total of $1,689,200,800 last year, an increase of 11 per cent over 1928; these classes of work usually increase when general business activity and the stock market are on the upswing; public and institutional buildings of all kinds (constituting the remainder of the non-residential classes) amounted to $901,202,200 last year, representing an 8 per cent decrease from 1928. Non-residential building as a whole had contract expenditures just 3½ per cent above those of 1928.

The decline was most severe in residential building, whose 1929 total was $1,915,727,500 being 31 per cent under the 1928 total. Even public works and utilities, contracts for which kept somewhat ahead of 1928, very nearly to the end of the year, finished with a contract total 7 per cent under that of 1928, or $1,248,342,000 as compared with $1,337,930,500."

Labored explanations do not make this essentially simple situation clearer. In a few years of over-rapid building we had exhausted both our current resources and our expanded credit resources. The majority of our families who could be persuaded to undertake paying for a home on our most persuasively stated installment plans, had been supplied. Investors, speculators, and coöperative owners had been furnished with apartments more ornate than ever before in exterior ornamentation and fitted with the lastest style of gadgets in interior decorations. Offices enough had been constructed to

3. F. W. Dodge Corporation.

house our high pressure salesmen and executives. And present and future needs for educational, commercial, and manufacturing buildings had been supplied. In the process the old sources of credit had been drained and the newly tapped springs had been exploited. By and large the process of expansion was complete. It remained only to care for the normal growth in population and business, to provide for some newly originated needs or to build for some overlooked nooks and corners of the industrial areas. We are better housed than ever before and the housing carries a heavier burden of debt than was true in any earlier generation. The first factor makes for comfort and efficiency; the second accounts for funds which under another plan of financing would have been expended in future building operations.

It adds little to extend this discussion. When the accounts are footed up we shall have learned new lessons respecting the evils of credit inflation. This dear bought wisdom we may place beside our knowledge of the evils of monetary inflation purchased at an equally dear price. And we may venture a pious hope that the joint lessons will induce growth of the wisdom to foresee, caution to move less rapidly and more surely in the path of progress, and acceptance of the fact that resources still limit the gratification of desires in the long run. A wired house does not mean — though sales literature assumes that it does [4] — that the family residing therein not only has electric lights and a telephone, but is also an early customer for an up-to-date radio, an electric washer, ironer, sweeper, heater, and oil burner. Many incomes can include but a tithe of these things, and no scheme of credit expansion can make it include them all. Nor can

4. Electrical Merchandising for January, 1930, p. 53. This gives a computation of the "degree of saturation" for the electrical appliances based on the assumption that each of these commodities may be placed in 100 per cent of the wired houses.

over expansion of credit be practised without dire penalties being visited on the family guilty of such excesses, and in case the mistake is widely indulged, on the industries generally and the nation at large.

This discussion should not be construed as an attack on, nor yet a criticism of, the new methods of credit sales. Mortgage bonds, building and loan financing of home ownership and installment selling will doubtless all prove useful and sound methods of financing. Their increasing use and their extension to income levels hitherto denied their advantages may well be justified by the increasing efficiency of credit rating, the new rapidity of communication and movement, and the rising intelligence and responsibility of the general public. The essential point here is that during the period of introduction of these new financial devices and while the newly opened reservoirs of credit are filling, we have a temporary increase in the nation's purchasing power. A combination of circumstances has rendered this expansion of large dimensions in the decade just closed. While the new credit is expanding to its justified limits, more than the normal amount of commodities may be sold. The industries affected are stimulated to expand their productive capacity and to build up their labor force to the level set by this temporary bulge in consumption. In the working out of competitive forces their great prosperity leads to expansion even beyond this limit as over-eager producers seek an enlarged share of the market and profits. Once the newly developed credit resources reach maturity, new debt created is balanced by installments due on previously assumed obligations. The nation can buy only such volume of goods as is covered by its current income. The check to expansion is sharp and is intensified by the excesses inevitably associated with periods of over-rapid expan-

sion. Such a course of events is clearly proven by the evidence as to credit expansion in the period 1920 to 1929. The depression into which the nation fell in the latter year was undoubtedly due in part at least to these developments in our complicated economic structure. Manifestly these events are too recent and our records too incomplete to attempt to measure their relative importance as compared with other factors of great weight. But there can be no doubt that their influence was large.

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INDUSTRIAL DIVERSIFICATION IN AMERICAN CITIES

SUMMARY

I. Advantages and disadvantages of industrial concentration. — Possible relation to the business cycle, 131. — II. The criterion of concentration and diversification here used: value added by manufacture, 134. — Results for sixteen cities in 1919, 135. — In later years, 138. — III. Relation between industries of producers' goods and of consumers' goods, 146. — Some significant results as to concentration and business fluctuations, 148.

I

THIS study has proceeded upon the hypothesis that there may be significant relations between the degree of industrial concentration in a given city and the business fluctuations which that city experiences. In recent years there has been a great desire on the part of many cities to diversify their industrial activities with the belief that variations in production and employment would be less severe. This desire was in part a result of the extent to which certain highly specialized cities suffered in the post-war depression. Questions immediately arise as to the relative advantages of industrial concentration or diversification, whether the business cycle can be smoothed by local readjustment, and what part diversification might have in smoothing out seasonal or cyclical fluctuations.

ADVANTAGES AND DISADVANTAGES OF INDUSTRIAL CONCENTRATION

Industrial concentration has made possible the development of the division of labor both in the mechanical arts and in business management. Furthermore, wherever a given industry is heavily represented, the

particular kind of skill required is easily attained by the workers. "The mysteries of the trade become no mysteries; but are, as it were, in the air, and children learn many of them unconsciously." [1] Invention is fostered; subsidiary trades in implements and materials develop. The community furnishes the industry with service well fitted to its needs. All in all, costs may be considerably reduced.

On the other hand, business fluctuations may be unusually severe in such a community. Andrew Carnegie early observed that steel in Pittsburgh was either prince or pauper. Concentration of a city's manufacturing in a few industries has the further handicap of employing, in the main, one group of laborers. In heavy manufacturing districts, for example, factories require mainly strong semi-skilled and unskilled workers. Unless other types of industry are introduced, the community finds it almost impossible to employ highly skilled labor or special labor reserves of women and juniors. The worker's family income may be relatively less; the payroll of the industry, relatively greater.[2]

It is not impossible to combine the advantages of variety of employment in different industries or in trade with those of the localization of industry. A city's economic structure can be well diversified and still include a sufficient number of mills working within a given industry to secure the advantages of division of labor, adequate supply of skilled workers, specialized service, and the like. There may even be effective diversification among the general types of occupation; as between trade, transportation, manufacturing, and the various service groups.

1. Marshall, Principles of Economics, eighth edition, p. 271.
2. Marshall discusses this problem with particular reference to steel districts; ibid., p. 272.

The business cycle is a composite of several lines of economic activity as they change from period to period. It is an average measured from some norm. Tho the business cycle registers prosperity, many businesses may be depressed, others extremely prosperous. True, there are broad economic forces which tend to depress or elevate all industry at the same time, yet some types of trade and production respond much more rapidly than others. In the post-war depression of 1921–1922 the United States Bureau of Labor Statistics Index of Wholesale Prices turned upward in January, 1922.[3] Cotton prices preceded this point by ten months; steel rail prices followed eight months later. Other low points were even more widely scattered.[4] Not only are there considerable variations in the timing of prices; differences in amplitude are fully as great. The price of crude petroleum varies twice as widely as that of bituminous coal and four times as widely as that of anthracite.

It follows that a community can stabilize its economic life in so far as it can find industries the fluctuations of which complement the fluctuations of existing industries, and to the extent that these desired lines of activity can be introduced. As a rule, since no two businesses have exactly the same seasonal and cyclical swings, the more types of production and trade are represented, the more stable will be that community's business. In Providence, a well-diversified industrial center, payroll earnings over the last three years have been remarkably

3. Mills, Frederick C., The Behavior of Prices, pp. 76–89, 512–518.

4. Indeed the very turning points of a business cycle depend upon what elements are included and their relative weights. The Harvard B curve touched bottom in January, 1922, but the American Telephone and Telegraph Company's index of general business conditions, constructed in somewhat different fashion, had turned upward six months previously.

constant.[5] Cincinnati's seasonal employment index is well known to vary much less than those of either Akron or Youngstown.[6] To what degree, then, are seasonal and cyclical fluctuations a function of industrial concentration?

II

In order to explore the relation between concentration and business variations it is first necessary to fix on some measure of the degree of concentration in a number of cities, as well as the seasonal and cycle variations in each case. As a measure of the degree of concentration in each city, it was decided in this inquiry to ascertain what percentage of each city's total manufacturing was accounted for by the five largest industries; and further, what percentage by the first twenty industries. Several criteria were considered for gauging the economic importance of an industry, such as number of wage earners, capitalization, value of products, consumption of power, wages, value added by manufacture. Some inadequacy was found with each of these, taken by itself, except with value added by manufacture. This was selected as the best measure; tho for use in other studies percentages of number of wage earners were also calculated.[7]

5. Brown Business Service, March 28, 1930, p. 40.

6. Not only is Youngstown's industry concentrated in steel, but two products dominate: pipes and automobile sheets. During the last two months of 1929, when the automobile industry's demand for sheets together with the pipe requirements of the oil industry dropped off, the steel industry in this district operated at only one-third of capacity. A less specialized steel district, such as that around Chicago, was operating at two-thirds of capacity.

7. Data have been taken from the Federal Biennial Census of Manufactures, 1919, 1921, 1923, 1925, and 1927. Value added by manufacture figures were not published in the 1923 report; however, for that year special figures for Pittsburgh, Detroit, and Cleveland have been obtained directly from the Bureau of the Census. In the case of most cities for some years, certain important industries have not been speci-

In 1919, the criterion selected (value added by manufacture) shows that in Detroit and Pittsburgh manufacturing was more concentrated in the first five industries than it was in any other large American industrial center.[8] As indicated in Chart I, over half of each city's manufacturing was being carried on in five industries.[9] In Detroit two lines of production, motor vehicles and motor vehicle bodies and parts, alone accounted for 38.3 per cent of the city's total. Pittsburgh's position in that year was largely accounted for by the steel and machine industries, which had expanded considerably during the war; since then blast furnaces and steel works and rolling mills have lost somewhat in relative importance. Youngstown, with nearly a third of its production in steel works and rolling mills, ranked third in concentration. New York City's high position resulted from the prominence of the clothing and printing industries. At the bottom of the list came Philadelphia. Buffalo, St. Louis, Newark, and Baltimore also appeared to have considerable diversification. Buffalo's position, it should be remarked, was not representative of the industrial concentration in that district, much of which in 1919 was located in adjacent towns.

If, instead of five industries, the first twenty be used as the measure for the fourteen cities, the ranking in

fically listed in the census reports. This is due to the fear of disclosing the operations of an individual firm. To supply these deficiencies, state census materials and special group estimates supplied by the Federal Bureau of the Census have been used. Throughout this study an industry has been used, with minor exceptions, as it is defined by the United States Bureau of the Census.

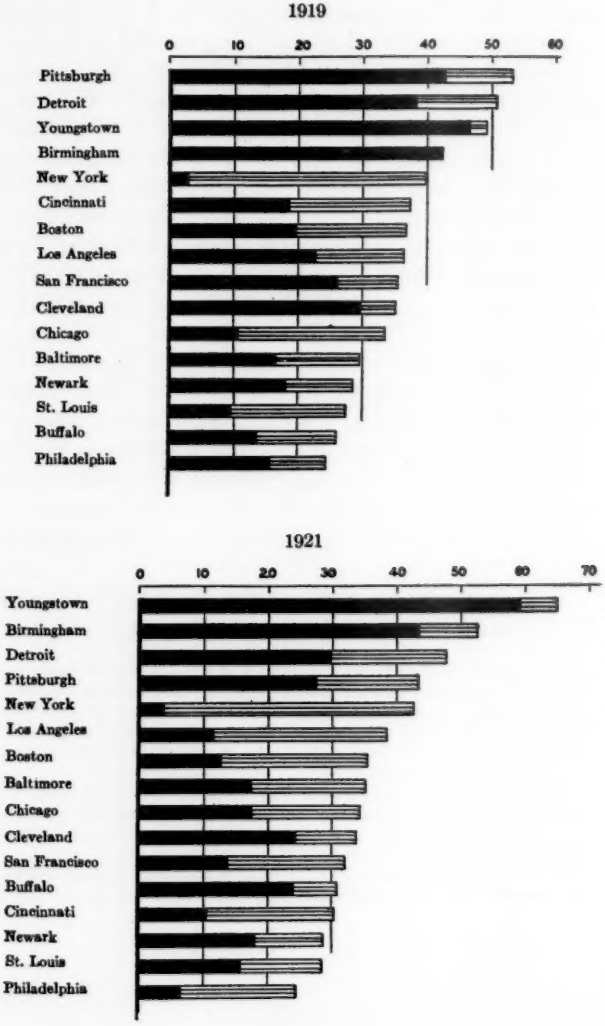
8. Fourteen of the leading American industrial cities have been included in these comparisons, together with two highly specialized steel cities, Birmingham and Youngstown. Statistics for these latter could be secured only for the five largest industries.

9. The four charts show also the relative importance of producers' and consumers' goods, which will be considered presently.

CHART I

PER CENT OF VALUE ADDED BY MANUFACTURE CONCENTRATED IN THE FIRST FIVE INDUSTRIES IN EACH CITY, 1919 AND 1921

(Producers' goods black, consumers' goods shaded)



1919 is much the same (see Chart IV).[1] However, in this case Pittsburgh, with 80.3 per cent of value added, clearly outranked Detroit, 72.2 per cent. The difference would seem to indicate that even tho concentration in Detroit's five great industries was slightly greater, her group of industries ranking below the twentieth was, nevertheless, of more significance. Another difference shown in the two measures for 1919 is of interest; for the more inclusive measure Los Angeles and Boston each ranked in concentration above New York City and very close to Detroit. It is somewhat surprising to note that Boston, usually considered a diversified center, should have had over 70 per cent of her manufacturing in twenty industries. The explanation here, as in

1. It is interesting to note the number of different industries in each of the sixteen cities during the five biennial years which this study covers. These data have been secured directly from the United States Bureau of the Census.

Number of industries in the United States and in sixteen selected cities

	1919	1921	1923	1925	1927
United States	358	348	333	324	335
New York	302	288	279	269	305
Chicago	291	281	250	263	275
Philadelphia	281	272	265	255	265
Detroit	198	179	178	180	192
Cleveland	225	211	203	193	210
St. Louis	217	215	212	200	216
Boston	242	220	...	207	201
Pittsburgh	167	170	160	152	153
Baltimore	213	192	184	182	190
Buffalo	211	203	198	189	183
Los Angeles	197	193	180	189	204
San Francisco	198	194	191	180	186
Cincinnati	217	199	192	199	203
Newark	219	205	203	193	197
Birmingham	84	74	81	81	80
Youngstown	68	55	52	49	51

The number of industries in the United States, as classified by the United States Bureau of the Census, has tended to decrease since 1919. It is well to bear this in mind in studying the figures for the individual cities.

The relatively small number of different industries in Detroit and Pittsburgh is some evidence of industrial concentration.

the case of Los Angeles, is, in part, due to the prominence in that year of the second ten as compared with the first ten industries. The printing industries — book and job together with newspaper and periodical — have included a rather large per cent of Boston's total value added: 14.5 in 1919; 18.4 in 1921; 17.1 in 1925; and 17.7 in 1927.

By 1921 Youngstown, with about two thirds of the city's value added in five industries, was by a wide margin the most concentrated of the sixteen cities. Birmingham had risen to second place. The rapid growth of iron and steel production in each of these cities accounted for much of their greater concentration. Among the larger cities the relative positions were much the same as two years before. Detroit, Pittsburgh, and New York City still presented examples of high concentration tion, tho the trend in Detroit and Pittsburgh, particularly the latter, had been towards diversification. Whereas in Detroit automobile industries had about maintained their relative importance, in Pittsburgh steel had dropped from 27.7 to 11.0 per cent of the total value added. Philadelphia in 1921 still had the least industrial concentration, tho its position was not greatly different from that of St. Louis and Newark.

Turning now to 1923 and later years, we find (with limited statistics) that by 1923 Detroit and Cleveland had become more specialized, altho the latter was still outranked by Pittsburgh (see Chart II). But with the first twenty industries as a measure, Cleveland for once ranked ahead of Pittsburgh. This was due to growth in the automobile industries; in fact 1923 marked the peak of such industries in Cleveland.

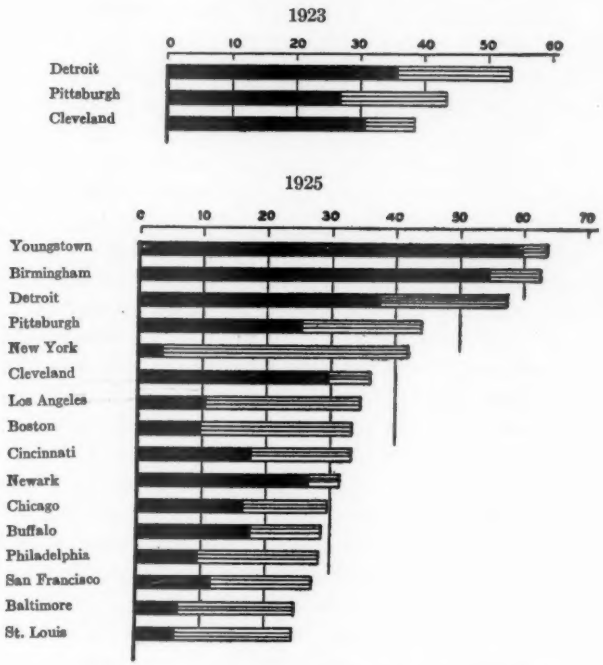
By 1925, Youngstown was almost as concentrated as it had been four years previously: blast furnaces and steel works produced half of the total value added by

manufacture. Third in rank for 1925 was Detroit with 58.5 per cent of all value added in the first five industries. The two automobile industries — finished

CHART II

PER CENT OF VALUE ADDED BY MANUFACTURE CONCENTRATED IN THE FIRST FIVE INDUSTRIES IN EACH CITY, 1923 AND 1925

(Producers' goods black, consumers' goods shaded)



motor vehicles and motor vehicle bodies and parts — produced 48.2 per cent of all value added. This percentage had been 38.3 in 1912, 35.4 in 1921, and 43.3 in 1923. The year 1927 — to anticipate for a moment —

was to see little change in the relative importance of these two industries, tho the value added in motor vehicle production appeared to be growing much faster than that in the production of bodies and parts. For 1925 Chart II shows Birmingham second, Pittsburgh fourth, and New York City fifth, each more concentrated than in 1921. Cleveland had risen in rank from tenth to sixth.

Baltimore's industrial structure had changed somewhat: a 35.1 per cent concentration in the first five industries in 1921 had dropped to a 24.9 per cent concentration in 1925. Shipbuilding, in 1919 and 1921 the city's second-largest industry both as to value added and as to average number of wage earners, had by 1925, according to each measure, dropped below twentieth in rank. The city's largest industry, men's clothing, decreased in relative importance from 1921 to 1925. Baltimore and St. Louis appear at the bottom of Chart IV as the least concentrated of the sixteen cities.

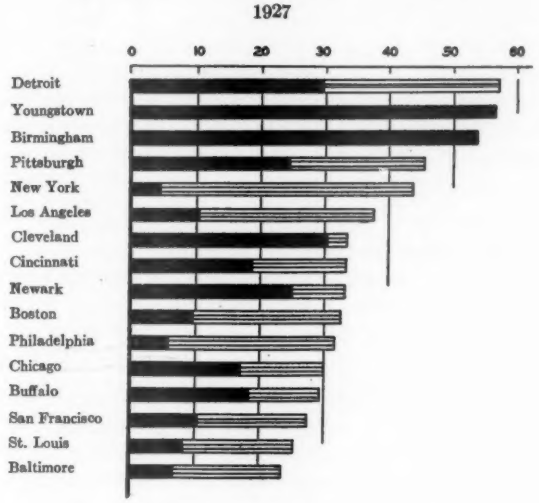
By 1927, Detroit had even more of its total value added by manufacture concentrated in the first five industries than did Youngstown (see Chart III). While in the latter the iron and steel industries had fallen in relative importance, in Detroit the two automobile industries had grown at exactly the same rate as the rest of the city's industries. The increasing importance of electrical machinery production had caused Newark to be more concentrated in 1925 and 1927 than in 1919 or 1921. St. Louis and Baltimore were again the least concentrated; in the latter, the largest industry had continued to decrease in relative importance.

For 1927, if the first twenty industries be used instead of the first five, the cities remain in somewhat the same order (see Chart IV). The more inclusive measure, however, gives Philadelphia, Boston, and Buffalo a

somewhat higher position. This is due in each case to the relative importance of those industries ranking from sixth to twentieth; for example, in Boston that year the ninth industry was almost as important as the sixth.

CHART III

PER CENT OF VALUE ADDED BY MANUFACTURE CONCENTRATED IN THE FIRST FIVE INDUSTRIES IN EACH CITY, 1927
(*Producers' goods black, consumers' goods shaded*)



All the tests show Detroit, Pittsburgh, Youngstown, and Birmingham to be cities of considerable industrial concentration. Youngstown was exceedingly concentrated in 1921 and 1925. In those years her five largest industries controlled 65.0 per cent and 63.3 per cent, respectively, of the city's total economic activity as evidenced by value added by manufacture. In 1921, steel works and rolling mills employed over half of all

wage earners. This is the clearest case of concentration disclosed. New York City is steadily more concentrated than Boston. Men's and women's clothing and the two printing industries account for more than one third of New York's production — in 1927, 40.0 per cent. The two printing industries lead Boston's manufacturing; in 1927, they produced 17.7 per cent of all value added. As measured by either the first five or first twenty industries, Boston and Cleveland are about equally concentrated, though in 1925 and 1927 the former is less so. Cleveland is steadily more concentrated than Chicago. Cincinnati follows in between Cleveland and Buffalo, being not quite so diversified as the latter. Buffalo, tho less dominated by a few industries than Baltimore in 1919 and 1921, was much more diversified than Baltimore in 1925 and 1927. As to the two California cities, Los Angeles is, each year, more concentrated than San Francisco.

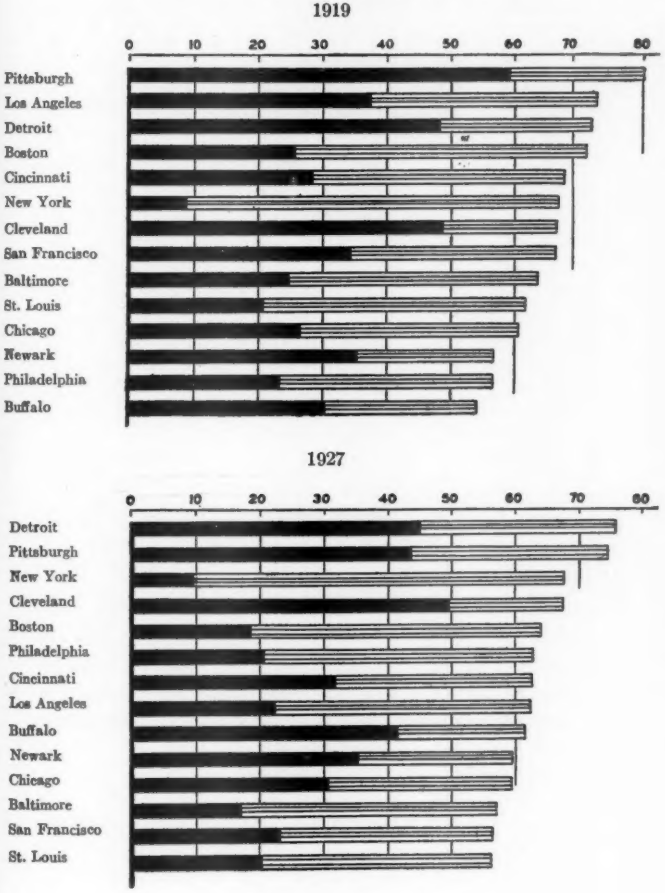
If only the one largest industry be compared, Youngstown always heads the list (Tables I and II). Her steel works and rolling mills average over one-third of the city's total manufacturing, even 41.0 per cent in 1925. Detroit is much less dominated by her first industry, motor vehicles. But the contrast is lessened if the two largest industries, not one only, be considered; for the making of motor vehicle bodies and parts, in the case of Detroit, is almost as important as that of motor vehicles. Pittsburgh's first industry is not of dominating importance after 1919. In Birmingham, cast iron pipe has, with the exception of 1919, occupied a high relative position.

In all of the years studied, Philadelphia was the least affected by its largest industry. Of the total value added by manufacture, shipbuilding accounted in 1919 for but 5.7 per cent. Two other industries were of

CHART IV

PER CENT OF VALUE ADDED BY MANUFACTURE CONCENTRATED IN THE FIRST TWENTY INDUSTRIES IN EACH CITY, 1919 AND 1927

(*Producers' goods black, consumers' goods shaded*)



nearly the same size. In 1921, Philadelphia was still the least dominated by a particular industry. In 1925 and 1927, St. Louis took Philadelphia's place; in the first year boots and shoes created 6.6 per cent of total value added; in the second, patent medicines and com-

TABLE I

PER CENT OF TOTAL VALUE ADDED BY MANUFACTURE CONCENTRATED IN FIRST INDUSTRY IN EACH CITY

	1919	1921	1923	1925	1927
1. New York City	16.0	16.3		16.9	18.2
2. Chicago	9.8	9.2		7.4	7.9
3. Philadelphia	5.7	7.0		9.6	10.8
4. Detroit	20.4	21.3	23.9	28.4	30.3
5. Cleveland	12.3	8.9	11.6	9.4	9.3
6. St. Louis	6.8	8.0		6.6	6.6
7. Boston	8.2	9.4		9.8	10.5
8. Pittsburgh	27.7	11.0	18.6	17.9	16.1
9. Baltimore	10.5	12.9		10.7	8.5
10. Buffalo	8.4	9.0		9.1	7.8
11. Los Angeles	11.8	13.4		11.9	14.4
12. San Francisco	17.0	9.5		8.7	8.8
13. Cincinnati	8.8	8.7		10.6	9.0
14. Newark	8.2	9.4		15.3	15.7
15. Birmingham	10.1	16.6		24.3	20.0
16. Youngstown	30.0	36.9		41.0	33.0

pounds the same per cent. Of the three inland cities, Detroit, Pittsburgh, Cleveland, the last named has been the least dominated by a single industry. Only Buffalo and Chicago have steadily held their largest industry at less than 10 per cent of total value added, tho in 1927 Boston's largest industry was but slightly over this mark.

TABLE II

THE LARGEST INDUSTRY IN EACH CITY AS DETERMINED BY VALUE ADDED BY MANUFACTURE

	1919	1921	1925	1927
1. New York City	Clothing, Women's	Clothing, Women's	Clothing, Women's	Clothing, Women's
2. Chicago	Slaughtering and Meat Packing	Clothing, Men's	Printing and Publishing: Book and Job	Printing and Publishing: Book and Job
3. Philadelphia	Shipbuilding	Printing and Publishing: Newspapers and Periodicals	Printing and Publishing: Newspapers and Periodicals	Printing and Publishing: Newspapers and Periodicals
4. Detroit	Motor Vehicles	Motor Vehicles [1]	Motor Vehicles	Motor Vehicles
5. Cleveland	Foundry and Machine Shop Products	Electrical Machinery	Motor Vehicles [1]	Foundry and Machine Shop Products
6. St. Louis	Boots and Shoes	Boots and Shoes	Boots and Shoes	Patent Medicines and Compounds
7. Boston	Foundry and Machine Shop Products	Printing and Publishing: Book and Job	Printing and Publishing: Newspaper and Periodical	Printing and Publishing: Newspaper and Periodical
8. Pittsburgh	Steel Works and Rolling Mills	Steel Works and Rolling Mills [1]	Steel Works and Rolling Mills	Steel Works and Rolling Mills
9. Baltimore	Clothing, Men's	Clothing, Men's,	Clothing, Men's	Clothing, Men's
10. Buffalo	Foundry and Machine Shop Products	Motor Vehicles	Motor Vehicles	Motor Vehicles
11. Los Angeles	Shipbuilding	Motion Pictures	Motion Pictures	Motion Pictures
12. San Francisco	Shipbuilding	Printing and Publishing: Newspaper and Periodical	Printing and Publishing: Newspaper and Periodical	Printing and Publishing: Newspaper and Periodical
13. Cincinnati	Machine Tools	Clothing, Men's	Clothing, Men's	Clothing, Men's
14. Newark	Electrical Machinery	Electrical Machinery	Electrical Machinery	Electrical Machinery
15. Birmingham	Foundry and Machine Shop Products	Cast Iron Pipe	Cast Iron Pipe	Cast Iron Pipe
16. Youngstown	Steel Works and Rolling Mills	Steel Works and Rolling Mills	Steel Works and Rolling Mills	Steel Works and Rolling Mills

[1] Also for 1923.

III

Relation Between Producers' and Consumers' Goods

The next step in the inquiry was to determine whether each of the sixteen cities was producing mainly producers' or mainly consumers' goods, or about equal amounts of both. For the purpose of this step the following classification was used. "Producers' goods" include goods which require further manufacture, as pig iron; completed articles which go into the assembly of a larger unit, as motor vehicle bodies and parts or structural steel; completed units used to aid production of another article, as warehouses or electric trucks used in factories; or completed units used in marketing finished articles, as a salesman's automobiles. "Consumers' goods" are finished articles ready to be sold and delivered to the final purchaser, who uses them for his own wants. Of course the same product may be either a producers' or consumers' good, depending upon the use to which it is put. Such is the case, for example, with automobiles and with furniture. Furthermore, a given census classification may include both types of goods. Hence it has been necessary in the case of certain products to make a rough allocation as to the extent to which a particular good is to be considered a producers' good or a consumers' good; thus, motor vehicles were divided two thirds consumers' goods, one third producers' goods.

Value added in producers' goods is much more likely to show fluctuations than value added in consumers' goods. From 1919 to the depression year of 1921, the former not only decreased with the drop in total value added, but decreased relatively to value added in consumers' goods. The change is more evident when the

first twenty industries are used (see Table III, but also Chart I). The greater decline in producers' goods was characteristic of every city studied except New York City, St. Louis, and Buffalo. True, the output of con-

TABLE III

PERCENTAGES PRODUCERS' AND CONSUMERS' GOODS OF VALUE ADDED BY MANUFACTURE IN THE FIRST TWENTY INDUSTRIES IN EACH CITY

P = Producers' Goods C = Consumers' Goods

	1919		1921		1923		1925		1927	
	P	C	P	C	P	C	P	C	P	C
	Per cent of Total		Per cent of Total		Per cent of Total		Per cent of Total		Per cent of Total	
New York City	15	85	15	85			13	87	13	87
Chicago	44	56	42	58			51	49	52	48
Philadelphia	40	60	32	68			35	65	33	67
Detroit	67	33	59	41	67	33	63	37	60	40
Cleveland	73	27	63	37	70	30	73	27	73	27
St. Louis	34	66	41	59			37	63	36	64
Boston	36	64	33	67			28	72	29	71
Pittsburgh	71	29	52	48	64	36	61	39	59	41
Baltimore	39	61	33	67			30	70	30	70
Buffalo	55	45	59	41			67	33	67	33
Los Angeles	52	48	39	61			38	62	35	65
San Francisco	51	49	42	58			43	57	41	59
Cincinnati	42	58	35	65			47	53	51	49
Newark	62	38	53	47			62	38	59	41

sumers' goods showed a relative gain in each city, but this was due primarily to the heavy drop in producers' goods rather than to growth of consumers' goods. On the other hand, when business moves out of a depression, producers' goods increase more rapidly. From 1921 to 1923 this was particularly true in Pittsburgh. When business dropped into a minor slump in 1927 the relative importance of producers' goods again fell somewhat in most cities. However, in Cincinnati, Chicago, and Boston the percentage of producers' goods in-

creased slightly. In both Cincinnati and Boston this was the result, in great measure, of expansion in the production of electrical machinery.

If only the first ten or first five industries be considered, the decline from 1919 to 1921 in the importance of producers' goods is relatively less. This is particularly noticeable in the case of Pittsburgh. The explanation is that most of the first five industries are producers' industries. Of course, where most of the larger industries produce consumers' goods, the relative decrease in the importance of producers' goods is much greater for the less inclusive measure; New York City is a case in point.

The great drop in value added by manufacture in 1921 for the highly concentrated cities suggests that there may be some relation between degree of concentration and the severity of cyclical, as well as seasonal, fluctuations. It appears likely that, wherever the concentration occurs in producers' goods, the cyclical drop will be more severe than where there is an equal amount of concentration in consumers' goods. On the other hand, it may be that consumers' goods are more affected by seasonal than by cyclical fluctuations.

The percentages of concentration for the fourteen large industrial cities in 1919 have been paired with the corresponding cyclical decreases in value added by manufacture from 1919 to 1921 adjusted for trend. These two were correlated, but the result revealed no important relation. Of no more significance was the correlation between the percentages of concentration in 1921 and the same cyclical decreases. The question then arose: is it not likely that those cities which produce mainly producers' goods are subject to more severe cyclical variations? To answer this question the

amount of value added by manufacture in the first twenty industries in each city was divided into two parts according as the value added represented the production of a consumers' good or of a producers' good. From these totals were calculated the percentages in producers' and in consumers' goods (see Table III). The percentage in producers' goods in each of the cities for 1919 was then paired with the corresponding cyclical decrease in value added from 1919 to 1921 adjusted for trend. These two series of data yield a coefficient of rank correlation of 0.88. This is significant. Taking the percentages in producers' goods in 1921, the coefficient was 0.93. These figures are evidence of a high degree of concomitant variation.

It is difficult to find an adequate measure of seasonal fluctuations in industrial activity — one which would give comparable results for each city. However, for a number of the sixteen cities comparisons can be made on the basis of data for month-to-month variations in manufacturing employment. An analysis of these showed, in the case of several cities, a significant degree of correspondence between concentration and the severity of seasonal variation. Thus employment in Youngstown fluctuates more from month to month than does employment in Cincinnati; Pittsburgh's seasonal variations in wage payments are much more severe than those in Philadelphia. These results do not warrant unqualified generalizations any more than do the figures given in the preceding paragraph; but they give food for thought, and point to the desirability of further inquiry on the relation between concentration and seasonal as well as cyclical fluctuations.

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MITCHELL'S BUSINESS CYCLES[1]

I

As far as it is the primary function of a review to draw to a book the attention of the scientific community and to tell by summary or critique what readers are to expect from it, this review is superfluous. Every economist knows Professor Mitchell's great book of 1913 and also the present instalment of the new monumental work. It does not want any introduction to the public, and is beyond any critic's power to help or to hurt. The only task left, it would seem, would be to enter into a discussion of the vast mass of important points of detail raised — a task to the fulfilment of which this book is fairly entitled both by its merits and by the eminence of its author, but which it is impossible to carry out within the compass of a review.

Yet every book of weight tells us something beyond what it has to say about its particular subject. It conveys necessarily a general message from author to readers about methods, horizons, aims and views, of which the treatment of the subject in hand is but an application or paradigma. If this be true of any book of stature, it is, in the economic field, especially true of books on the problem of business cycles, which by its very nature calls for a display of practically all the powers and acquirements of the author and unavoidably bears, by implication at least, upon every other department of our science. One of the best things said

1. Business Cycles; The Problem and Its Setting, by Wesley C. Mitchell, with a foreword by Edwin F. Gay, New York, National Bureau of Economic Research, Inc., 1928.

in the volume before us, is the suggestion (p. 452), "that ideas developed in the study of business fluctuations may lead to reformulations of economic theory." It is indeed obvious that in dealing with business cycles we are dealing with all the most fundamental elements of the economic life of capitalist society, and are sure to meet practically all our great problems on our way. Rarely, if ever, has any worker in this field made so full a use of the possibilities thus offering themselves as Professor Mitchell has, and it is this aspect of his work on which primarily I shall undertake to comment.

First, we cannot be too grateful to the eminent author for rigorously brushing from his path idle controversy on fundamental principles of "method," which lingers in this field longer than in any other. The reader has but to look at pp. 469–474, where there is a resumé of Professor Mitchell's views on this point, in order to be assured that he will be able to follow the argument without feeling in duty bound to turn from relevant discussion and to send in some grand remonstrance on such topics as the respective merits of collecting facts, of statistical treatment of facts, and of handling them by that kind of refined and systematized common sense, which we have come to call "economic theory." Professor Mitchell's every line is, needless to say, well nourished with every kind of available fact, including his "own store of experiences and observations"; he is "ready to apply the mathematical technique of statisticians"; and he proposes "to guide our statistical investigations by rational hypotheses," repudiating the "error to think that free use of factual materials reduces the need for careful reasoning." Guarded and judicial as these utterances are, they cover in their admirable brevity and simplicity a great deal of ground. They are infinitely valuable, when said ex cathedra by a promi-

nent leader in a field in which they are as yet less universally accepted than in others and which still suffers from a belief, harbored by many ardent workers, that explanation can ever come to us as a by-product of mere collection of facts and that it spells scientific sin to use, in dealing with these facts, any logical tools not at the command of the "man in the street." Nor is this all. *Via facti*, by starting in his first chapter with a survey of the work done since the discovery of the problem as well as of the solutions now current, he testifies to his belief in the continuity of science, thereby condemning implicitly any program of "starting anew" and that attitude which takes a pride in mere lack of scientific training. His teaching on this point may be summed up, I believe, saying that any "New Economics" can come about not by program but only by achievement.

True, Professor Mitchell does not take kindly to what savors of "theoretical construction," and his preferences, while toned down both by scrupulous fairness and by his invariable courtesy and generosity, are yet clear enough. He can hardly help associating the work of theorists with insufficient command over facts and with ways of thinking which seem to him backward. Even in cases in which an author's command of facts was no more backward than his analytic apparatus, he would stress the former and underrate, as it seems to me, the importance of the latter point; just as, on those few and well-timed occasions when he is looking for formal analogy to the procedure of physical science, he seems to overstate the importance of the experimental, and to understate the importance of the theoretic side of their work. In places he forgets or denies that there is such a thing as theoretic proof or disproof of a proposition, and seems to consider "theories" as so many suggestions of

which one is really as good as any other before being put to the decisive judgment of statistics. I confess to some doubt, whether every one of the authors considered in the first chapter will recognize themselves in the pictures drawn of them. But this matters little. In spite of it he is leading up to common ground, on which we may hope to work harmoniously towards the common goal, even tho the task being a complex one and calling for very different mentalities, aptitudes, and likings, we may never be able fully to appreciate each other. To quote a colleague of mine, professor of Experimental Physics at the University of Bonn: "If I call in a theorist to hear what he has to say about the results of my experiments, and if my man shows so much experimental aptitude as is implied in being able to switch off the light, I begin to doubt his competence as a theorist." Yet he calls him in.

There is another comment I should like to make upon the work as a whole or its "general message," which extends the importance of the book far beyond the precincts of the mere problem of business cycles. Professor Mitchell, as he has told us somewhere, is no friend to systematic treatises — which most of us will probably agree to be in some respects necessary evils. But as a matter of fact, he has written one or, at all events, sketched out the fundamental contours of one, with such unmistakable clearness, that any competent economist could supply most of the rest without risk of serious error. I think it safe to say that no more than one fifth of the book before us bears specifically on business cycles in the sense that it has not just as much to do with any other group of the great problems of our science — distribution, or pricing, or monopoly. Now, inasmuch as we have before us the work of one of the leading figures of the scientific world of economics and of a leader who

has, in former years, often betrayed some displeasure at the state of our science and our ways of dealing with its problems, both in toto and on a number of well-defined headings, it seems but natural, and hardly unfair, to ask what the outstanding features are, which could be pointed to as constituting or implying fundamental differences in results or horizons, and whether the presence of such differences spells revolution or evolution. It is natural, in trying to answer this question, to think of that mighty structure which, tho battered in places by the impact of newer methods and results, still stands broadly in the background of much, if not most, of the best work of our day — Marshall's great treatise.

There is undoubtedly a difference in aim and character. Marshall's fame and influence rest on his mastership in constructing tools of analysis, on his having built, out of the material of the theoretic ideas of his time, an engine of analysis. It is the fifth book of the Principles (and matter placed elsewhere in the treatise which really belongs in that book) which is immortal in the sense in which scientific achievement can ever be called immortal. Now Professor Mitchell, being of the experimentalist type, would never undertake a similar task; he would consider it, as all experimentalists do, of secondary importance; he thinks of theory not as an analytical engine, but primarily either as a store of rational hypotheses, or as a body of doctrine or as an arsenal of generalizations gleaned from arrays of well-digested facts. He even would never, I presume, preface factual analysis by an elaborate theory as, to quote another instance, Professor Taussig has done in his International Trade. He would think theory in the Marshallian or Walrasian sense just as little worth while for its own sake as he would refinements on statistical methods which have no immediate bearing on prob-

lems in hand. But, again, just as he is not prepared to forego the use of the tools put at his command by modern statistical methods or to condemn the use of all of them except the elementary ones, so he would not and could not deny — he would in fact have no logical standing ground to do so — that science cannot progress, after a certain stage has been reached, without the construction of tools of thought different from those of every day life, growing up as the result of conscious effort: *id est*, without a theory, which has as little to do with metaphysical speculation or political doctrine as the discussion by the pure theory of mechanics of "possible" forms of movement, and which is not an unscientific or provisional substitute for facts, but an instrument — spectacles, so to speak — needed in order to discern the facts.

This being the case there is a great difference in emphasis, and yet no epistemological gulf between Mitchell and Marshall; not even between Mitchell and Marshall's fifth book (appendix included). If difference there be, it can only be found under one of the following headings, in none of which it spells break or revolution.

(1) It can be held, that, while theoretical tools are a necessary evil, yet all the tools so far devised are vitiated at their root by some initial error, *e.g.* a fundamentally false psychology. To which I should reply, that, however faulty we may think the psychology of economists to be in dealing with such problems as property, taxation, motives of enterprise or saving and so on, our tools of analysis such as quasi-rent, equilibrium, coefficients of production, *even* marginal utility can be interpreted so as to tally with *every* kind of psychology. The psychological background is, so far as any point of this kind is concerned, little more than a façon de parler

and hence cannot possibly be the logical — as distinct from historical — derivate of any particular one.

(2) Without attacking fundamentals there is, of course, plenty of room for difference as to the usefulness or otherwise of any single tool or method — that is, any way of handling a given set of facts for given purposes. For myself, I confess to a strong belief, for example, in the future rôle, both within theory itself and in the practical problems of "Welfare Economics," of the tool called "sum total of consumers' surpluses." Perhaps Professor Mitchell does not share this belief. But if we were to discuss the question, we could not but discuss it as a question of theory and by means of theoretical arguments. Epistemology and methodological creeds would not enter. Such differences, necessarily incident to scientific work, are no cleavages between "schools."

(3) But must the tools of theory not prove useless for the book before us, being constructed without reference to its task — a *numerical* treatment of our problems? But have they been thus constructed? Have those who went before us not all theorized with an eye to quantitative [2] treatment, present or future? In Marshall's case especially, Mitchell, the fairest of critics, surely would not deny that he both saw and worked for, this task. We need not read between the lines in Marshall or dwell on the explicit statement in that great manifesto of his, "The old generation of economists and the new." We need only look at the text, at the treatment of demand or of cost, at the distinction between external and internal economies, the theory of monopoly, the dealing with the element of time, the formula of point elasticity — all opening their arms, as it were, to future masses of statistics. Nor need we speak of his reiterated hints at

2. "Quantitative" and "numerical" are synonymous in Mitchell's book.

the stores of statistics of all kinds already available. It is true that, to take the simplest instance, his demand curve has repeatedly been denied the aptitude to serve as the demand curve of statistics. Yet it has been the beacon to all work in the field, and whether it will or will not prove useful in the end is still *sub judice*.

(4) Finally, our analytic engine, it is held, is bound to undergo continuous change, fully as much as mere "hypotheses" are, under the influence of the results of "factual" study. It can never stand by the side of them as if an immovable organon of hyperempirical canons. Not only must the stream of new fact present to us ever new problems necessitating, to use a phrase of Pigou's, the attaching of new arms to our engine; but also, and much more important, increasing insight into facts necessarily makes some instruments of theory obsolete and creates the need for new ones. As Mitchell says a propos of a special case (p. 54): "But as our knowledge grows wider and more intimate, our attitude toward the discussion of causes undergoes a subtle change." Of course it does. The theoretical part of every science always refers to, and acquires its meaning from, a given state of factual knowledge. The instruments of theory which have been useful, and theorems which, in the sense of pragmatism, have been "true" in one state, may very well, and often *do*, prove bars to progress and even downright wrong in another. New theories and new criteria for the acceptance of theories become inevitable as time and work go on. They would be necessary even if the old ones had been perfect for their day. A single fact à la Michelson may change the face of theory. But *quis negavit*? Difference on this head can arise only if "factual" students deny that the influence of factual study on the apparatus of theory is no greater than the influence of theory on factual study; if they overlook the

necessity of continuously directing part of our collective effort toward improving our theoretic tools as such, independently of the impulse of new facts or problems; if they refuse on general grounds to acquire a working knowledge of them; and if they reject particular instruments in cases in which it is doubtful whether they have fully mastered their meaning.

II

If I have succeeded in scaling down to a difference of what might be termed the scientist's personal equation, what at first sight appears to be a fundamental difference of worlds between the book Marshall *has* written and Mitchell *might* have written, it becomes easier to indicate the other characteristic features of our author's hypothetical "Principles." There are the institutional element, the statistical element and (following Professor Mitchell's caption) what we will call "the contribution of business annals."

The institutional contribution (Chapter II, on Economic Organization), could be incorporated into a general treatise almost verbatim. Its bearing on the particular problem in hand is but indirect. Unless acquainted with the embers of recent controversial flames, no reader would think of the names of Veblen or Schmoller in reading it. It is this to which I want to draw attention. I believe it to be no part of our duty to the memory of those men to try to overlook the serious and even glaring defects in their equipment, both natural and acquired. On the contrary, justice toward them requires us to mention them precisely in order to explain, and thereby to excuse, part of what they said. He who insists upon taking at face value certain tenets of theirs, casts an undesirable imputation on their men-

tal power. Schmoller was undoubtedly a great man. Nobody would like to do without the fruits of his positive work on institutional history. No economist thinks, or ever thought, these things superfluous or secondary. It is with part of his technique of economic argument that we find fault. Here it is impossible to deny either that he was far from perfect or that he exerted an influence which is in part responsible for what everyone — especially every German — admits to be an unsatisfactory state of economic science in Germany. Veblen differed from Schmoller in very many points. But as far as economic reasoning goes, much the same applies to both. Had he been able to have his way, had his teaching not met a phalanx of competent theorists, we should perhaps have to make a somewhat similar statement as to America. Now Professor Mitchell's pages are remarkably free from any tendency to *substitute* institutional investigation to economic theory. They are entitled, here again, to the tribute that while they show an open mind to whatever valuable contributions may be expected from that quarter, they are resolutely shut to the misconceptions which have often impaired their usefulness. In some of the sections of this chapter, conspicuous among which is the one on "guidance of economic activity," there is nothing but matter for cordial endorsement.

Critical comment, then, can only bear on particular points. The prominence given e.g. to the monetary aspect of business phenomena is but natural from either of two viewpoints. One of these, however, implies provable error, which I am far indeed from imputing to Professor Mitchell. Whoever is guilty of the erroneous view that economic life is changed to its very core by the intervening of money, that a money economy must be explained on principles differing *toto caelo* from

those applicable to a non-monetary life, or that, finally, when pointing to the difference between making goods and making money, he is pointing to a fundamental cleavage — would obviously be justified from that point of view in so emphasizing that aspect. The other viewpoint is that *calculating in money* gives to our economic behavior a measure of precision which it would not otherwise display, and that it rationalizes economic life, and, further, life in general. The use of money may well be looked at as one of the great moulding influences of civilization, both in the sense of a cause and in the sense of a vehicle of other causal elements. This is correct, but more relevant to sociology than to economics. And if we avoid the error just mentioned, and if the other statement, tho true is one with which economics is little concerned, then it becomes surely more useful to base "The Meaning of Business Economy" (p. 63) on our old friend, the division of labor, of which the practice of "making and spending money" is nothing but a technical consequence.

Again, the section on the system of prices may well be accepted as a translation into non-technical language of part of the fundamentals of theory of the Walrasian type. But it may equally well be accepted as an example of what is lost in the process of translation. Nicer questions, more recondite interrelations are absent. No guiding principle is provided, in the face of the fact, that, the interdependence between *all* prices being recognized, it would be logically impossible for the author to deny the existence of such a principle. The intending "factual" student goes without the help of so much as "elasticity," "cost-curve" or "quasi-rent," which he not only ought to, but in our days actually tries to, fill with numerical material. The forms of interrelations between prices are of many kinds. Theorists, such as

Edgeworth and Fanno, have attempted to work out some of them and the result of their labors is essential for many things, among them an understanding of the details of what happens during business cycles. But we do not hear of them. Nor could it be pleaded that, the interrelation among prices being taken simply as a fact of experience, whatever more may be to it will automatically show itself in the process of enlarging that experience and sifting further data. This hope must fail, for the same reason which makes direct and mere observation of the phenomena displayed by patients inconclusive and untrustworthy — because some of the interrelations among symptoms may be, and generally are, overlaid by others. If there be anything less than admirable in the two highly instructive sections on the monetary mechanism and the flow of money payments, I believe it to be traceable to the eminent author's dislike of the theoretical apparatus. It is, however, more important that there is nothing in this chapter — or in any other — which could be considered as incompatible with the apparatus of theory; nothing which could either be proved to be wrong by it or prove it to be wrong in toto or in part. As in every point of detail, this may be seen in Mitchell's treatment of the fundamental concept of equilibrium (p. 186). We might plead with him as to the justice of calling it a mechanical analogy, and try to win his assent to the view, that the equilibrium of a balance sheet is not, as he holds, "a different conception of equilibrium" but one special case of it, and we should perhaps demur to the statement that we have simply (p. 187) no more warrant for assuming that business processes "tend" to maintain an equilibrium than to assume that they "tend" to get out of balance. To most of us it seems that the one assumption applies to certain business processes and the other to different

ones and that it is important, again in the interest of factual study, to distinguish between the two. We should also like to have Professor Mitchell's view on the usefulness or otherwise of some related "tools," among them that called "representative firm." Finally we should have been grateful to learn more than we do (p. 376) about how "equilibrium" stands to that "figment" of a normal state of trade. Yet the idea itself is there and brought into definite relation to factual study, from which we may in future discuss it — and it is this what matters.

III

Fully 170 pages out of the 474 in the book are none too many to deal with the momentous matter of Chapter III, the "Contribution of Statistics." Many of us will echo Professor Mitchell's criticism of the current distinction between statistical and theoretical work. It is here that the economics of the future will differ most significantly from the economics of the past. The salient point is not in the increasing wealth of statistical information or in an increasing readiness to use it. Nobody who weighs the methodological implications of Ricardo's reply to Mr. Bosanquet, can deny that on principle economists have always been ready to use what "factual" information was at their command, or that the difference in this respect is no greater than the difference between the theoretical apparatus of the classic and the modern theorist. The point is rather that we are increasingly learning to *think* in terms of statistical quantities [3] and with a view to the fact that our data offer themselves to us mostly in the shape of time series. And here I beg to echo Mitchell's call for closer coöpera-

3. An important instance of what I mean is the present process of remodeling the Theory of International Trade by means of the concept of the Barter Terms of Trade.

tion from the other side of the fence, or, as I should rather say in view of what Mitchell himself tells us on the subject of trends (pp. 221 and 230), to add emphasis to an element of the case already emphasized by him. It is not only that economics has to learn from the statistician's approach and, incidentally, to struggle with its static fetters, but also that statistical methods will have to be remodelled at the suggestions of economic theory and to get as much as possible over its own static fetters, which are no less stringent than ours.

Some of us still look upon the store of methods in modern statistics as if they were, like Calculus, wholly unconnected with any particular subject and equally applicable to all. There exists a body of general theorems and there are pieces of mathematical technique of which this is quite true. And on the strength of this there is a tendency to proceed to calculating averages, indices, coefficients of correlation, trends, deviations from trends, as if the methods used would, if only correctly applied, of necessity yield significant results, and as if the choice between alternative methods were merely a matter of statistical convenience, or, at best, of applying formal criteria, such as the absence of freakishness in results, sensitiveness to influences obviously relevant, or, again, stability, reversibility, closeness of fit, and so on. But it has long been pointed out that this is entirely inadmissible. Whether we are measuring, as we mostly do in Physics or Astronomy, one and the same magnitude, or a type; whether the type is a constant one or variable in time; whether we do or do not theoretically know something about the form or law of its variation — in every one of these cases we are faced with different problems, to which different rules must apply. Even a mere arithmetical average, or its standard deviation, is perfectly meaningless unless we know be-

forehand, whether there is some "norm" in the set of data we have to deal with and what the nature of that norm is.[4] I am very far from imputing to Professor Mitchell any errors on this score, yet I confess to a feeling that in perusing Chart 24, Fig. R, which presents the duration of 106 (!) cycles in what looks like a frequency polygon — with standard deviation and all — I have missed any warning to the effect that this figure has no statistical meaning whatever. It has none, because the norm of the phenomenon under consideration has obviously changed and the criteria as to what is to be called a "cycle" are neither definite nor uniform enough to warrant one in speaking in this case of a statistical "universe," of which the 106 cycles could be considered as a sample. "Static" assumptions are no exclusive *privilegium odiosum* of economic theory. What I am concerned with here is the fact that only theory — in the boldest acceptance of the word — can tell us anything about the presence and in the end about the nature of a norm [5] or, more generally, of a "Kollektivgegenstand" in the sense of H. Bruns. Further, the statistical formula we apply can only grow out of the theory of the subject we have, if it is to have definite meaning. Any example will show this. We have, e.g. been constructing indices of "general prices" these many years and discussing their respective merits and shortcomings without working out their theory, i.e. without settling what it is they are to measure. All the inconclusiveness of the discussion and all the difference

4. It may still be useful to look up what Lexis says on this point, in the article on Anthropologie and Anthropometrie in the Handwörterbuch der Staatswissenschaften.

5. To be sure, the theoretical norm is a thing differing in its very essence from the statistical norm. Yet it embodies what may be called the "meaning" of the latter — its theoretical soul, as it were — and this meaning must be settled before we can proceed to construct a statistical norm and to put it to any scientific use.

of opinion about the tests to be applied in judging alternatives, is traceable to this source — and disappears immediately, when we tackle the problem from the theoretical side. It is easy to do this in this case: Writing the Walrasian equations of the equilibrium, we find that we have one less than unknowns. We usually mend the case by putting one of the p's equal to 1. It is better to mend it by giving the Σpq of consumer's goods an arbitrary value. As we could assign it any other value just as well, we may also say that we are introducing an arbitrary "factor of proportionality." This factor is the exact kernel of the vague idea of "level of general prices," and it is its changes we are seeking to extricate from the changes in individual prices when we try to measure changes in general prices. And as soon as this is understood — and no sooner — we derive a formula which has a title to being called *the* correct one.[6]

Again, we have indeed become more critical about the coefficient of correlation, and it is being well nigh universally recognized, that $n = 0.999 \ldots$ is compatible with the absence of any real relation, $r = 0$ with its presence. The relation may be overlaid by other influences or simply have some other form. It is less universally recognized that this tool grew out of a soil very different from ours, and that by using it we are introducing the whole of the assumptions of the theory of errors — of

6. Denoting by E the sum spent on consumers' goods, and by $p_1, p_2 \ldots p_m$ the prices, by $q_1, q_2 \ldots q_m$ the quantities sold at the time t, similarly by $E + dE$, $p_1 + dp_1 \ldots p_m + dp_m$, $q_1 + dq_1 \ldots q_m + dq_m$, expenditure, prices and quantities sold at the time $t + dt$, we have $dE = \sum_1^m pdq + \sum_1^m qdp$. As it is easy to see, the price-level would be constant if $\sum_1^m qdp = 0$, and the deviations from zero of this expression are a measure of its changes. Of course, the formula which we get from this applies only to infinitesimal changes from point to point, but it gives us a standard by which to judge every other one. It is readily seen that Professor Fisher's ideal formula comes out well.

"static" statistics — which may well reduce our results to meaninglessness. To be sure, we need not, and cannot, let the matter rest there. Noteworthy attempts have been made to get over these limits of that important tool — e.g. by Oskar Anderson (*Die Korrelationsrechnung in der Konjunkturforschung*, Bonn, 1929). But it remains true that we must carefully scrutinize the justification we have each time we use it. And, here again, the construction of the statistical tool can only come out of previous theoretic analysis of the case in hand.[7]

Nowhere are considerations of this kind of such importance as in the case of trends. There would be little overstatement in saying that trend-analysis will be the central problem of our science in the immediate future and the center of our difficulties as well. It is here that our contention will be most readily conceded by the statistician. He is so often being forced to discard "unbelievable results" that it is easy — or ought to be — to show that an inspection of results from other than statistical standpoints imposes itself upon us not only in cases of *glaring* absurdity of results, but in *all*. This is only another way of saying that if trend-analysis is to have any meaning, it can derive it only from previous theoretical considerations, which must not only guide us in interpreting results, but also in choosing the method. Failing this, a trend is no more than a descriptive device summing up past history with which nothing can be done. It lacks economic connotation. It is, in fact, merely formal. We can apply the familiar methods just as well to e.g. a few succesive years of a pros-

7. This is my objection to most of those textbooks of statistical method which undertake to give the economist "just what he wants for practical work" and which start with comforting him that they will not lead him into the redoubtable thickets of mere mathematics. They can hardly help misleading him. For there is no understanding of our tools without a full grasp of their mathematical implications.

perity-phase, as to the whole of the material we may happen to have (as, again, to a period of political commotion). The result has the same claim in every case to be called a trend in the statistical sense, and may in each case be decomposed into component elements in an indefinite number of ways which have no rational connection to each other — unless it be supplied by the theory of the subject under research.[8] As soon as we speak of "the" trend of a series, *a fortiori* if we follow Professor Mitchell in dignifying it with the term "secular," we are theorizing by implication; and there is no bolder theorizing than that which works up from impressions or propositions not explicitly stated. We cannot be too grateful to Professor Mitchell for discouraging exclusive reliance on the test of closest fit. The trends we want are very different from those we get by fitting a curve through unanalyzed material.[9] But this opens up a host of questions, for example, the question barely touched in the volume before us, whether it is the trend which is the "generating" phenomenon of cycles or the cycles which generate the trend; whether or not the trend is a distinct economic phenomenon at all, attributable to one factor, or a well-defined set of factors; whether all the points on our raw graphs have on principle equal right to exerting an influence on its slope, and, if not, which credentials we are to ask of everyone point before admitting it. Now, inasmuch as there has been much work on a number of some of these factors — cp., for instance, the string of analyses Professor Pigou presents in his Industrial Fluctuations — we might hope

8. These considerations are entirely independent of — altho practically reinforced by — the shortcomings of the methods actually in use, which it is impossible to enter upon here.

9. To have to change one's trend in the lapse of time is no misfortune, but what we should generally expect to happen. But misfortune there may be in not being able to account for it theoretically.

that "factual study" would step in to grip our data with these newly forged tools. That is what the "experimentalist" worker undoubtedly would do in every other science. Not so on our field; here this blessing seems slow to come. Yet it should not be difficult to show that keeping distinct such things as (*a*) *that element of growth, which is capable of being decomposed into infinitesimal steps* — such as increase in population or savings; (*b*) the effect of industrial and commercial innovation; (*c*) influences from outside the economic system — such as harvests, gold discoveries, wars, changes in social organization or in the attitude of men towards business success — and the working out of theoretical schemata for every one of them, might go a long way towards answering many of the questions alluded to, and give additional definiteness both to the aims and results of "realistic study." It is in this direction that we have special reason to look forward eagerly to what Professor Mitchell is going to tell us in the next volume.

IV

I have the same to say on the Contributions of Business Annals. The National Bureau of Economic Research has put all of us under obligation by offering us so handy a work as Dr. Thorp's. There can be no doubt that the *histoire raisonnée* of cycles is to the student of cycles what "mother earth" was to Antaeus. It is, moreover, a source which opens up aspects inaccessible to mere time-series-analysis, just as it needs, in its turn, reinforcement by monographical research into single industries and, in many cases, individual concerns (such as we may hope to get from the series of monographs Spiethoff is starting at present). But looking at the use made by Professor Mitchell of the material collected

under his auspices, I cannot help being struck by what seems to me an excess of caution traceable to a reluctance to let himself be served by theory — not as *hypothesis*, but as a *tool*. He even refrains (p. 450) from attempting to draw up a full catalogue of the factors obstructing the tendency to international synchronization of cycles, which would be one of the main contributions to expect from intensive "factual" study. In such matters as e.g. the cause of difference in cycles in different countries, he does not care to link the difference in the character of the "turning points" in England on the one side, and Germany and pre-war America on the other, with the difference in relative capital-wealth, a relation suggested by theory and signally verified by facts. Surely we have no quarrel with such results as that cycles are becoming increasingly internationalized by "the endless series of actions and reactions among the influences exerted and experienced by all the nations. . . ." (P. 447.) Yet most of us will come to the conclusion that the Annals, while undoubtedly enabling us to see more clearly, have not so far helped us to see farther.[1]

Again, doubts about the fruitfulness of the Annals *without* the aid of theoretical groundwork, as distinguished from their fruitfulness, if used with it, arise occasionally, where the attempt is made to make them yield additional information of significance. Professor Mitchell, in dealing with the duration of cycles, finds, for instance, many more of them than his predecessors used to. But this is simply due to the lack of a sufficiently definite principle in deciding what is to be called

1. Comparison, too, suggests itself with the results of the life-long, tho single-handed, labors of Spiethoff, and doubts arise as to the advisability to confine coöperation with him to a hardly satisfactory reference in the introductory chapter.

a recession. We could easily find still more cycles, and without theoretical aid there is no logical resting-place until we get down to those ripples which everyone knows are common even within a year of clearly marked boom or depression. To quote another instance, Professor Mitchell considers at length, and with qualified and guarded approval, Professor Mills' hypothesis on the changes in the length of cycles. With due respect to Professor Mills and the eminent services rendered by him to our science, I am at a loss to account for this preference except on the ground that that hypothesis is *not* theoretically founded. Else it is hard to understand how the difficulty of verifying it, the arbitrariness incident to it, and such blemishes as that it makes Austria after 1873 rank with old industrial countries saturated with capital, like England or the Netherlands — how these things can have failed to put it out of court, when confronted with the proposition of the "period of gestation," which theory would suggest and which Mr. Robertson has made a promising first attempt to verify — an attempt banished by Professor Mitchell to a modest place in his introductory chapter.

It may be the theorist's egotism or prejudice which accounts for my impression that, to conclude, the main direct contribution of this volume towards the solution of the problem, with the "Setting" of which it deals, suffers from the same cause. This contribution consists in the attempt to establish cycles as a "valid" (= real?) species of phenomena (p. 383). Having very properly followed Juglar in discarding the surface-phenomenon which we loosely call a "crisis," Professor Mitchell defines cycles as recurrences [2] of depression, revival, pros-

2. He might safely have called then "periodic" recurrences. The objection to the adjective has no other foundation than that every day parlance does not recognize "periodicity" except periodicity which displays either a constant period, or a period obeying a definite mathematical law.

perity, recession, and defends them against the attacks of Dean A. B. Adams and Professor Fisher (p. 464 *et seq.*) — with the question of phases to be distinguished he deals as if this were a mere matter of convenience. He even emphasizes their existence as a phenomenon *sui generis* somewhat at the expense of the "Long Waves" and the so-called "40-months cycle," altho the existence at least of the former is not only equally well established as that of the cycles themselves, but may well turn out to constitute the really significant problem, compared with which cycles may move to the background in much the same way as crises had to as soon as the cyclical movement was discovered. He seems, however, forced, by his method of approach, to stress so exclusively the formal requirement of recurrence as such, that the ground gained is largely lost thereby, and that his grip on his subject relaxes to the point of feeling (p. 396) "that we have no warrant for discarding cases in which cycles have been cut short or prolonged by wars . . . *or any other factor* (my italics) unless we believe that such disturbing circumstances will not recur in the future as in the past." Well, even earthquakes recur, and as we should have to extend the principle just quoted to cases which are not only cut short or prolonged, but also *caused* by wars and similar influences, we stand to lose the contours of our phenomenon. Incidentally we should lose much of what is so valuable in Professor Mitchell's separate treatment of irregular fluctuations (p. 249, *et seq.*). In fact, we should be on the road to giving up the idea of the cycles being a "valid" phenomenon.

All this is of secondary importance. Criticism is apt to loom unduly large in a review, and may do so in the present discussion. If so, I apologize and express again gratitude to the eminent author and admiration for his

work, which must ever stand out as a landmark within its field and far beyond it. Such difference in outlook as remains between us can be summarized by saying the theory of the cycle is not the last but the first step on the road to our goal.

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